Vol. 49, No. 5 September, 1942

Psychological Review

EDITED BY
HERBERT S. LANGFELD
PRINCETON UNIVERSITY

CONTENTS

Raymond Dodge: R. S. WOODWORTH 395

The Theory of Order: WILLIAM R. SICKLES AND GEORGE W. HARTMAN. 403

An Approach to the Study of the Social Act: MAPHEUS SMITH 422

The Psychology of John Dewey: PAUL CRISSMAN 441

Level of Aspiration as a Method of Studying Personality:
JULIAN B. ROTTER 463

Semiotic and Psychological Concepts: L. O. KATTSOFF AND J. THIBAUT. 475

The If-Then Relation and Scientific Inference:
JAMES A. CHRISTENSON, JR. 486

'Reminiscence' in the Studies of Professor English and His Associates:
CLAUDE E. BUXTON 494

Reminiscence—Reply to Dr. Buxton's Critique: HORACE B. ENGLISH .. 505

PUBLISHED BI-MONTHLY
BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STS., LANCASTER, PA.
AND NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS OF

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

PSYCHOLOGICAL REVIEW

HERBERT S. LANGFELD, *Editor*
Princeton University

Contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.
Subscription: $5.50 (Foreign, $5.75). Single copies, $1.00.

PSYCHOLOGICAL BULLETIN

JOHN E. ANDERSON, *Editor*
University of Minnesota

Contains critical reviews of books and articles, psychological news and notes, university notices, and announcements. Appears monthly (10 issues), the annual volume comprising about 665 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.
Subscription: $7.00 (Foreign, $7.25). Single copies, 75c.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

S. W. FERNBERGER, *Editor, on Leave,* FRANCIS W. IRWIN, *Acting Editor*
University of Pennsylvania

Contains original contributions of an experimental character. Appears monthly (since January, 1937), two volumes per year, each volume of six numbers containing about 520 pages.
Subscription: $14.00 ($7.00 per volume; Foreign, $7.25). Single copies, $1.25.

PSYCHOLOGICAL ABSTRACTS

WALTER S. HUNTER, *Editor*
Brown University

Appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.
Subscription: $7.00 (Foreign, $7.25). Single copies, 75c.

PSYCHOLOGICAL MONOGRAPHS

JOHN F. DASHIELL, *Editor*
University of North Carolina

Consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.
Subscription: $6.00 per volume (Foreign, $6.30).

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

GORDON W. ALLPORT, *Editor*
Harvard University

Appears quarterly, January, April, July, October, the four numbers comprising a volume of 560 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.
Subscription: $5.00 (Foreign, $5.25). Single copies, $1.50.

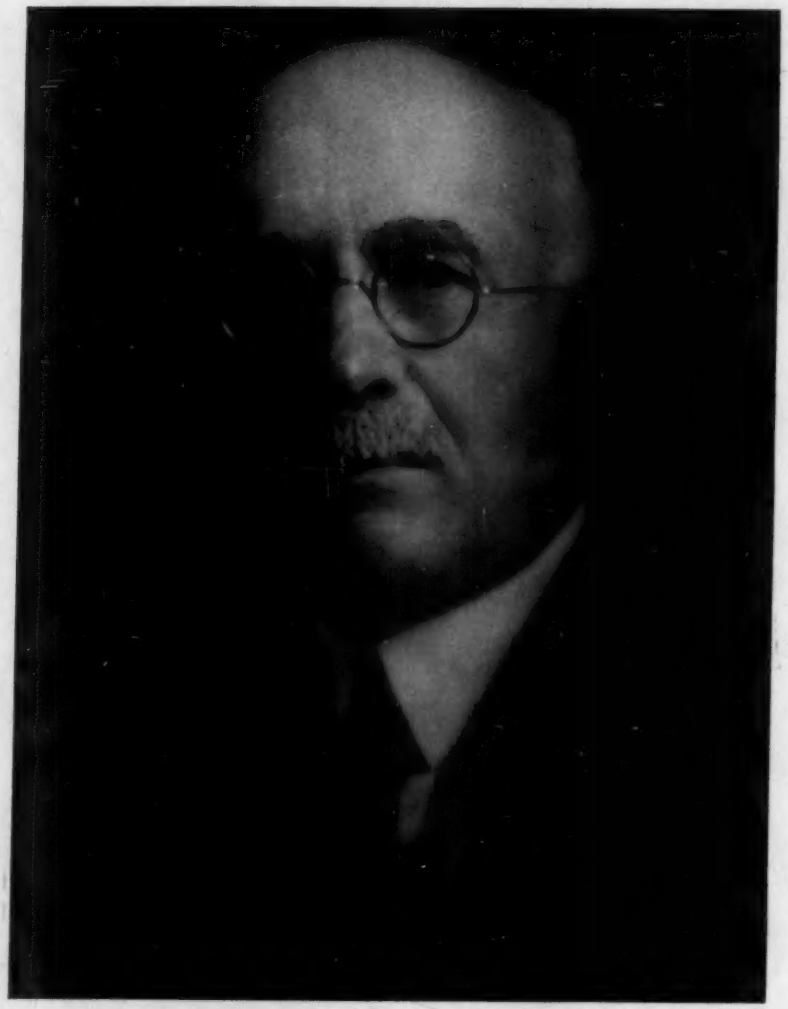
COMBINATION RATES

Review and Bulletin: $11.00 (Foreign, $11.50).
Review and J. Exp. (2 vols.): $17.00 (Foreign, $17.75).
Bulletin and J. Exp. (2 vols.): $18.50 (Foreign, $19.25).
Review, Bulletin, and J. Exp. (2 vols.): $23.00 (Foreign, $24.00).

Subscriptions, orders, and business communications should be sent to

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS



Raymond Dodge

VOL. 49, No. 5 SEPTEMBER, 1942

THE PSYCHOLOGICAL REVIEW

RAYMOND DODGE

1871–1942

In Raymond Dodge the older generation of psychologists has lost one of its most distinguished members, and the one of us all, probably, who was most ingenious and productive as an experimentalist. The number of difficult technical problems he solved is remarkable, each technical problem arising from some scientific problem which he wished to solve with as much precision as possible. He doubtless had the makings of a great engineer. His skill in mechanical construction dated from boyhood, but during his college course at Williams his interest centered in philosophy and he remained there for a graduate year devoted to this subject and then made his way to Germany to continue his philosophical studies under Benno Erdmann at Halle. Erdmann, however, had definite psychological interests as well, and some of them were susceptible of experimental attack if only suitable apparatus could be designed. In Dodge's *Autobiography* there is an interesting story of how he turned from philosophy to psychology. Erdmann in a perfectly friendly way expressed his doubt that Dodge would ever go far as a philosopher, and at about the same time Erdmann was wishing for a tachistoscope which should expose all parts of a word simultaneously—a requirement which he believed could not be met. "My initiation into experimental psychology," says Dodge, "may be said to have started with that technical problem. I adopted it as my own, lived with it, and gradually evolved the Erdmann-Dodge tachistoscope. . . . My engineering of apparatus has always proceeded in much the same behavior formula—a sort of mental trial and error of available ex-

pedients . . . , followed by rough construction and subsequent refinement where necessary." [1]

By aid of the tachistoscope Erdmann and Dodge (1) conducted some very incisive experiments on the process of reading. They disproved the old theory of letter-by-letter reading and proved the importance of the total word form as a cue for word recognition (confirming and amplifying the earlier results of Cattell). They also, by use of a mirror to facilitate observation of the reader's eyes, discovered the saccadic eye movement in reading (in this they were anticipated to a certain extent by Javal). They showed that the effective visual stimulation occurred only during the fixations and that the saccadic movements simply had the function of shifting the eyes as rapidly as possible from one fixation to the next.

The study of eye movements called for something better than unaided observation. It called for registration of some kind, as was indeed seen by several experimenters at about the same time. Dodge brought this problem back to America in 1897, where he taught at Ursinus College for a year and then at Wesleyan University for many years. He saw that some form of photographic registration was indicated and after some difficulty developed the corneal reflection method, which maintains its preëminence to this day. Soon he was able to identify 'Five types of eye movement' (2)—the saccadic and the pursuit movement, convergence, the reflex compensatory eye movement from stimulation of the semicircular canals, and the back swing of the eyes which maintains fixation during head movement. This fundamental finding has held good for the past forty years, though Dodge himself and others have since added much to our knowledge of these various eye movements.

Eye movements had played a prominent role in the older theories of space perception as well as of reading, and the theorists had freely assumed such eye movements as their theories demanded. With the establishment of definite facts

[1] *History of psychology in autobiography* (C. Murchison, ed.). Worcester: Clark University Press, 1930, Vol. I, pp. 102, 103.

regarding eye movement the theories had to be revised and indeed the revision amounted to a revolution. Dodge himself was among the first to point out the theoretical implications of the new experimental findings.

The tactile-kinesthetic or eye-movement factor in the visual perception of space and movement had been accepted as important and very strongly emphasized by many psychologists. Dodge devoted particular attention to this problem. He wrote in 1904 (3, pp. 2, 3):

> Recent experimental study of the eye movements has discovered a new and serious ground of suspicion against the traditional importance of the motor data by exposing the poverty and inaccuracy of our direct apprehension of the eye movements. The continuous involuntary eye movements, when the eye seems to the subject to maintain a constant fixation; the fixation pauses which interrupt every natural sweep of the eye across a complex field of view; the discrete corrective movements at the end of every considerable eye sweep; in fact, most of the known characteristics of the eye movements yield no introspective data at all, or only such as are ambiguous or absurdly inadequate. It scarcely seems probable that eye movements which we cannot even count, of whose amplitude we have no immediate subjective clue, of whose very existence we are often not aware even under the most rigid self-observation, could be very important factors in the perception of minute spatial changes. Not only, however, is there no independent consciousness of the eye movements, adequate to the refinement of the visual perception of motion, but the character of the eye movements which occur when we view a moving object furnishes evidence that, if our consciousness of them were complete and exact, it would be either useless or misleading as a datum in the visual perception of motion.

A little later (4, p. 67) he pointed out similar difficulties confronting 'the traditional motor theory of retinal organization.'

> Probably the most conspicuous result of all the recent exact data with respect to the eye movements is the evidence they force upon us of the utter insufficiency of the ocular kinesthetic data. Neither the velocity nor the extent of our eye movements is directly known to us. We have no immediate consciousness of their direction. Usually we are unaware of their existence. All this evidence comes

as a shock to the self-complacence with which eye movements and eye strains have been exploited by the introspectionists.

Admitting, however, that conscious sensations of eye movement might be unnecessary, provided the eye movements themselves had the proper characteristics to furnish the basis for visual space, he went on to show that the movements altogether lack the fine precision of the visual perception of space. Fixation is never perfectly steady, and refixation of the same object seldom brings it back to exactly the same part of the macula.

. . . , the traditional assumption of an identical anatomical and functional center of the retina, . . . is untenable.

There is no punctiform functional center of the retina . . . differentiated from all other retinal points as the point of clearest vision, to which all other retinal points are related by a system of motor impulses, which are calculated to bring every point of interest to the fixation point, . . . (4, pp. 21, 23, 26).

Spatial discrimination outside the area of clearest vision is much more precise than the movements which bring a peripheral object to the fovea. In short, the 'spatial organization of the retina' is much finer and more exact than can be accounted for by the eye movements. In this 1907 paper Dodge offered a developmental theory of spatial organization in terms of the recurrent similar stimulation of neighboring retinal elements, but this theory has not been taken up to any extent. He admitted that the organization was probably in large measure innate.

The word 'organization' occurs very often in Dodge's earlier papers (5), being replaced later, in large measure, by 'systematization.' In his early emphasis on 'word form' and in his persistent attack on problems of organization he showed some kinship with Gestalt psychology, and indeed he said in 1931 (13), "I like to feel myself a part of the Gestalt movement, a behavioristic Gestalt, notwithstanding Köhler's argument against behaviorism." His line of attack was however altogether different from that of the Gestalt group who have apparently found little of interest in his contributions. As regards behaviorism, Dodge was one of those who, before

Watson, came out clearly for the importance of non-introspective methods in psychology. He devoted a paper in 1912 to this matter, concluding as follows:

> In view of all the evidence I believe that introspection is only one of the indicators of mental reality. It is a real and important indicator of peculiar value in special fields but it is only one of many. Equally real I believe is every pathological or neurological fact, every result of practice, training or fatigue that throws any light on mental capacity, mental organization, or mental defects (6, p. 229).

Since Dodge did not rule out introspection altogether and in fact made effective use of it in some of his numerous experiments, he obviously cannot be classed as a strict behaviorist. He liked to call himself a devotee of 'dynamic psychology' and was one of the first to use this phrase (7). He meant by it, particularly, a study of the *conditions* determining behavior, experience and personality. The variability of behavior and experience and the factors in such variability seemed to him especially worthy of attention. Recognizing the importance of individual differences, he concentrated his own efforts on the study of the variation that appears in the same individual as a function of known conditions. Some critics who have apparently only a superficial acquaintance with his work assume that he was contented with establishing the mere fact of variation. Nothing could be farther from the truth.

One of his studies of 'variability' (12) examined the effect of repetition and practice on selected responses at different levels of the nervous system. There were about 80 experimental days spread over a period of 20 months. The knee jerk was elicited over 1000 times in the course of the experiment. There was no question of conditioning the knee jerk to some new stimulus. The stimulus remained the same throughout, but the question was whether the latency or amplitude of the knee jerk would show any practice effect. There was no change in amplitude but there was a progressive increase in latency. The eyelid reflex to a noise showed little change in latency but a pronounced decrease in ampli-

tude—a definite habituation effect or negative adaptation—and this effect was still more pronounced in a still higher-level reflex, the post-rotation nystagmus of the eyes (10). As against the a priori assumption that reflexes were too stable to be affected by learning Dodge thus demonstrated a definite practice effect, though in the negative direction. Cortically controlled responses such as finger tapping or word naming showed a positive practice effect as was expected. In considering whether these two ostensibly contrary practice effects must not have a common factor, Dodge pointed to the important role of the elimination of superfluous movements or acts in positive learning.

Another factor in which Dodge took great interest was fatigue. In the complex processes studied by the psychologist under the head of mental work he showed that the output was no true measure of work done by the brain and that the output decrement was no true measure of cerebral fatigue. "It seems to me to be a serious common fault in all the familiar fatigue experiments that we have developed no adequate technique for controlling . . . the inner stimulus, the degree of effort and incentive" (7). True fatigue may be compensated for by increased effort, while a deceptive work decrement may result from boredom, from competing interests, or from the need of progressively more rigid control as the subject becomes warmed up to the details of his task. Using changes in pulse rate as a rough indicator of metabolic changes he obtained evidence that the mental work in taking a written examination was greater during the period when a question was being read and its implications grasped than during the writing of the answer. In his Presidential Address in 1916 before the American Psychological Association (9) he suggested two 'Laws of Relative Fatigue.' The first: "Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the stimulus," *i.e.*, particularly, of the 'internal stimulus.' And the second: "In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from the competition in favor of the less fatigued tendencies." The

fatigability of a tendency was probably dependent on the physiological factor of 'refractory phase,' and a study of this factor might lead to an understanding of negative adaptation and other effects of practice and repetition.

Conflict of tendencies was made visible in the photographic records of eye movements obtained in the technically most remarkable of Dodge's experiments (11). Seated on a rotating platform the subject tended to make compensatory or rearward reflex eye movements from stimulation of the semicircular canals, but for part of the time he was exposed to a visible environment which, instead of moving normally to the rear, moved forward faster than he was himself being rotated and tended to carry the eyes forward. Here as in other experiments Dodge's purpose was to find simple instances of dynamic relations which were technically open to exact study. "The important generalization is not from simple to complex behavior . . . but rather that formulas of apparently widespread application to human behavior can be studied and recorded in such relatively simple phenomena as the movements of the eyes" (13). So the pursuit movement of the eyes in response to a swinging pendulum—an adjustment which is pretty well perfected within a few swings—was used to bring out several important characteristics of the learning process: approximate adjustment followed by correction, anticipatory reaction, and the interrelation of total configuration and striking detail.

During his long stay at Wesleyan, from 1898 to 1924, Dodge developed many of his principal experiments and trained a number of assistants who later became prominent in the psychological world. One year of this time, 1913–1914, was devoted to a study of the psychological effects of moderate doses of alcohol, in collaboration with the physiologist, Francis G. Benedict, at the Nutrition Laboratory in Boston (8). For another year, 1922–1923, he was Chairman of the Division of Anthropology and Psychology at the National Research Council in Washington. During the first World War he was one of the many psychologists who contributed to the war effort, and his contribution was unique in that he

was an officer in the Navy. He devised a very successful method of selecting and training naval gun pointers and also made psychology serviceable in the antisubmarine warfare. In 1924 he became an active member of the new Institute of Psychology, later the Institute of Human Relations, at Yale, where he remained till his retirement in 1936.

Though Dodge was so intense and independent a worker, he was also a loyal and effective adviser and collaborator; and though his standards of scientific work were high and amply justified him in trenchant criticism of experimental flaws, there was never any bitterness or professional jealousy in his criticisms. He was always looking for good wherever he could find it. His pupils were devoted to him and with good reason. His colleagues admired and loved him and gladly admitted that he, fully as much as any one of his generation, was winning for psychology a respected position among the natural sciences.

REFERENCES

1. Erdmann, B., & Dodge, R. *Psychologische Untersuchungen über des Lesen auf experimenteller Grundlage.* Halle: Niemeyer, 1898. Pp. 360.
2. Dodge, R. Five types of eye movement in the horizontal meridian plane of the field of regard. *Amer. J. Physiol.*, 1903, 8, 307–329.
3. ——. The participation of the eye movements in the visual perception of motion. Psychol. Rev., 1904, 9, 1–14.
4. ——. An experimental study of visual fixation. *Psychol. Rev. Monogr. Suppl.*, 1907, 8, No. 4 (Whole No. 35). Pp. 95.
5. ——. A working hypothesis for inner psychophysics. Psychol. Rev., 1911, 11, 167–185.
6. ——. The theory and limitations of introspection. *Amer. J. Psychol.*, 1912, 23, 214–229.
7. ——. Mental work: a study in psychodynamics. Psychol. Rev., 1913, 20, 1–42.
8. ——, & Benedict, F. G. *Psychological effects of alcohol.* Washington: Carnegie Institution, 1915. Pp. 281.
9. ——. The laws of relative fatigue. Psychol. Rev., 1917, 24, 89–113.
10. ——. Habituation to rotation. *J. exp. Psychol.*, 1923, 6, 1–35.
11. ——. Adequacy of reflex compensatory eye movements. *J. exp. Psychol.*, 1923, 6, 169–181.
12. ——. *Elementary conditions of human variability.* New York: Columbia University Press, 1927. Pp. 107.
13. ——. *Conditions and consequences of human variability.* New Haven: Yale University Press, 1931. Pp. 162.

R. S. Woodworth

Columbia University

THE THEORY OF ORDER [1]

BY WILLIAM R. SICKLES

Cooper Union for the Advancement of Science and Art

AND

GEORGE W. HARTMANN

Teachers College, Columbia University

I. Introduction

Since the very beginning of speculative thought, man has sought to define himself—to find some constant relationship between the shifting phenomena of life and the semi-permanent environment. Out of this search have come science, religion, and philosophy. Almost every major thinker has dealt, at one time or another, with the time-honored problem of the objective-subjective relationship. It forms a constantly recurrent theme in the Upanishads, a part of the ancient Vedantic literature of India. In China (4th Century B.C.), we find the philosopher Chuang Tzŭ nicknamed 'Butterfly-Chuang' because of his attempted solution of the difficulty plus the realization of his failure.

Virtually all theorists have had difficulty with the objective-subjective problem. Such was also true of the founder of psychophysics. Fechner, a professor of physics at Leipzig, turned to philosophy and came to the conclusion that mind and matter are identical. Out of a desire to prove this identity scientifically and in a manner satisfactory to himself as a physicist, he evolved his psychophysics and gave mathematical expression to the relationship discovered by Weber. His entire program of psychophysics was worked out and published (1851) in the *Zend-Avesta*, a volume named after the ancient Persian sacred book which advocated a religion

[1] The position here outlined was originally sketched by the first author (Sickles); the second author (Hartmann) is responsible for some of the stylistic features and incidental content and for further developments in the main conception of this paper.

with a basic metaphysics not too dissimilar from that held by Fechner himself.

It is not our intention here to discuss the Weber-Fechner Law—no doubt familiar to all who read this article—or the criticism it has received from Delboeuf's time to the present. What we are concerned with at the moment is that Fechner sought after, and felt he had discovered, a constant relationship between the objective and the subjective worlds. As a philosopher, he realized that such a constancy and such a relationship were necessary as a fundamental framework if there was to be any scientific legitimacy to the theoretical and experimental offspring of the 'new' psychology of his day.

Psychologists and philosophers are at present very much divided on the psychophysical problem, some even denying that it is a meaningful problem. Carr (1) lists three typical viewpoints on this issue. He uses as an example of the problem of constant relationships the question of how many spatially discrete objects the subject can visually apprehend (or organize) simultaneously. Both Pillsbury and Titchener had said only four or five. Carr terms the attitude which leads to the search for such relationships the 'Constancy Attitude' and asks: "What shall we do with these numbers when we get them?" Of course, the reason such numbers and values are (despite their industrial utility) next to worthless in providing theoretical suggestions and hypotheses leading to further experimentation is that they are 'peripheral' problems which throw little or no light on the basic nature of the mechanisms involved. The opposite of this attitude, Carr calls the 'Relativity Attitude,' *i.e.*, one which seeks to determine the conditions to which the variations in number are relative. Again he asks: "What shall we do with this list of efficacious and non-efficacious conditions when we get them?" Once more there is no satisfactory answer. As with the previous approach, the difficulty is that such investigations deal with 'dissociated' problems without any underlying hypothesis to determine or organize the variables involved. Finally, Carr states: "*Perhaps it is the relations that are constant*" (1, pp. 514–515; italics ours).

II. Order as a Psychological Problem

It is with this statement that our paper starts. Though we do not mean here exactly what Carr meant by *relations*, we do hold that it is the relations which are constant. The 'constancy' between the organism and its environment exists in the *methodology of behavior*, *i.e.*, the conditions of all behavioral interaction—conditions necessitated by the nature of the organism as such. The study of order is the study of the principles underlying the orderliness of behavior. This article contends that in the final analysis such principles dissolve into the fundamental laws of electricity and electromagnetism.

This is no new or startling hypothesis. With some reservations one could trace it back to Heraclitus, the 'Nous' of the Ionian school, or to von Ehrenfels (3). However, the idea that behavior in all its details may ultimately be wholly dependent upon the action laws of electricity, *is* somewhat new. In his recent volume, *Dynamics in psychology*, Köhler sees the possibility of psychological activity involving electric currents (5). Speaking of the tendency of a figure to block its own process, he notes: "Percept processes here show a behavior which is obviously analogous to that of currents in electrolytes. Can it be that percepts are actually associated with electric currents in the nervous system?" (5, p. 74). Later, however, he discounts this possibility on the basis of the *small* intensity which such activity would involve, saying: "The fact that all electric phenomena in the brain are of slight intensity tends to exclude electromagnetism and electrodynamics" (5, p. 115).

At the time of the publication of this work of Köhler's, one of the authors (Sickles) was experimenting independently along these same lines. This research, subsequently published, suggests that *actual* electric currents are operative in the visual field (10). The phenomenal distortion apparent in the Ponzo illusion results in a curve startlingly similar to that calculated, and thereby predicted *a priori*, from the progressive alteration of the electro-magnetic field values.

Our thesis is that what we know phenomenally as *order* is really the manifestation of underlying electrodynamic laws governing behavioral processes. We shall cumulate the supporting evidence indirectly before presenting the culminating claim at the end of this discussion. Dewey and others have declared that mind is essentially a new and higher level of organization or order (**2**). But this does not noticeably advance our understanding because *nobody seems to know what constitutes order per se.* Order forms such an integral part of our existence, both organic and inorganic, that it has never been fully examined. People have taken it for granted. True, its importance to objective reality has been recognized from the time Aristotle first posited the idea of 'Form.' Scientists at all times have used the terms 'form,' 'organization,' and 'order' in many broad senses to fit some descriptive or explanatory requirement. But only within the last two or three decades have psychologists of the Gestalt school revived the problem in a search for the principles underlying the realization of form as a quality of a perceptual whole. Unfortunately, the concepts so derived are numerous, confessedly vague, and in themselves unexplainable or at least irreducible to more familiar notions. Neoconfigurationism must correct the inadequacies of existing configurationist thought.

The simpler exterior connections between order and mentality are self-evident. The presence of order in the object of experience is a condition prerequisite to sensory perception and remembrance. Its absence leaves confusion, chaos, doubt. For example, the mind groups digits and nonsense syllables, and by such grouping remembers them with greater efficiency. Wundt revealed that the same was true of metronome beats. Many neurologists feel that all the qualities man attributes to his environment depend chiefly upon just such grouping.[2]

[2] As soon as one accepts the All-or-None Law, the 'systemic' nature of the determinants of conscious qualities is inescapable. Adrian has demonstrated that frequency and time are the only two variables in the action of an individual nerve fiber and that these both vary directly with stimulus intensity. Nafe says, "The experimental evidence as it now exists forces the hypothesis that all differences in common

Wherever man has gone or whatever he has done, his first task has ever been the evolving of some kind of order, organization, or system out of an earlier 'chaos.' Strictly speaking, this means the production of a higher degree of order out of a lower level of order. Only through the presence of this factor of order can man cope with his environment and give it meaning. This is so well known among philosophers that it is virtually a truism. For example, we find Spinoza explaining order thus: "When things are so placed that, if they are represented to us through the senses, we can easily imagine them and therefore easily remember them, we call them well arranged; but if they are not so placed that we can imagine and remember them, we call them badly arranged or confused" (11, p. 263).

Order is as much a fundamental psychological problem as it is philosophical or physical. If we hope to understand psychological activity we must dissect and probably revise our concept of order; for it is upon order that both sensory experience and memory depend. The very words 'order' and 'form' by their unexamined obscurity will forever stand as barriers to any truly scientific investigation of the organism unless defined by critical analysis. How shall we, *e.g.*, explain the 'grouping effects' of the neural impulses—the terms in which Matthews and others account for the genesis of qualities? How shall we explain the experienced aesthetic value that accompanies certain orderly arrangements of our sensory fields, the inability to remember disorderly things, or the order which accompanies the everyday actions of the individual and the community?

That order is a psycho-geometric problem is of great significance to Gestalt psychology, the only psychology which seeks to base the behavior of the organism upon principles of

sensation depend upon quantitative, *i.e.*, temporal and spatial, differences in the patterns of a single fundament, the nervous impulse. . . . The result for our theories is to make experience not a matter of the addition of impulses from certain types of nerves, but a matter of the arrangement of impulses in a system of nerves" (7, p. 1078). —The suspicion that experienced qualities depend upon the pattern (grouping, arrangement, or order) of the neural discharge is, therefore, well in keeping with the views of Matthews and others, and also underlies the volley theory of audition.

form. But, as others have asked before, what is this configuration? When we have explained this, we shall have explained order. Vernon asserts: "It is the mutual relationship of the physical stimulus qualities in the mysterious organization known as 'form,' 'structure,' or 'configuration' which is the essential objective determinant of most percepts" (12, p. 56). This 'mysterious organization,' as we shall see, is nothing less than *order*.

III. Nature and Genesis of Order

Our first task is to analyze order, to find wherein it consists. We look at an orderly arrangement and remember it, but we have much difficulty in remembering that which is relatively disordered and can do so only by prolonged effort. Even this attempt fails if the object or event is too complex. The degree to which we are able to remember it is directly proportionate to the degree of order it possesses. On inspection, it will be found that *order depends upon the even repetition of a unit in itself orderly*. Consider a checkerboard. The squares are of equal size and spaced an equal distance apart. The color of the squares is alternately the same. The unit, the repetition, the design, and the whole are evenly arranged. Therefore, we say the checkerboard has a high degree of order.

However, if we take the squares of a checkerboard and give certain of them unequal dimensions, the order is broken and a new difficulty presents itself, *viz.*, to remember the whole we must now remember also the degree of variation of these particular squares. Since we find that the design, as a part of an observable whole, can be broken up into smaller designs by altering the sequence of their units, *i.e.*, making the distances between like parts irregular, we discover that one of the basic elements in the orderly whole is the repetition. It is the repetition which defines the unit. Repetitions must be equal and occur at regularly spaced intervals.

Thus we arrive at the last step in our analysis—the interval. There must be more than one interval to allow for comparison, because the distance between any two points cannot be judged without another arbitrarily selected distance

somewhere to be used as a standard of comparison. Like the yardstick it all depends on what was first chosen to be called a yard. To have comparison, we must have a reference system. To obtain this, we must place another point either between or to the side of the two original points to provide a second interval. *Any given line between any two points, when devoid of all other determining lines or points, is an impossibility, a non-existent thing possessing neither qualitative nor quantitative characteristics.* It can neither be straight nor curved, because it lacks determining coordinates.

The positing of a coordinate system common to all astral movement in space-time is the problem with which the Theory of Relativity deals. Bertrand Russell in his exposition on relativity declares: "Between two distant events, there is not any one definite interval" (**9**, p. 223). As Koffka realized, two individual points do not yield a coordinate system, but only one dimension without direction, distance, or orientation.

In connection with this point, it is interesting to note an experiment suggested by Witasek to determine whether retinal points have fixed spatial values, *i.e.*, 'local signs.' Put your subject in a dark room, said Witasek, and have him fixate a single point of light. Then expose other points and let him indicate their direction from the original. But, as Koffka remarks, Witasek's experiment can never be performed. Why? "If you stay for any length of time in a totally dark room with only one point of light visible, this point will soon begin to move around in the most erratic manner, making excursions up to 90 degrees. . . . These 'auto-kinetic' movements, then, prove that no fixed retinal values belong to retinal points; they produce localization within a framework, but do so no longer when this framework is lost" (**4**, p. 213).

Of course, in our world of experience, it is utterly impossible to obtain two perfectly isolated points. Any line, *in vacuo*, has to be determined by at least three points in order to have any characteristics whatever. Nothing has characteristics in and of itself alone. Comparison and judgment are necessary, *i.e.*, standards must be present. Hence, three

points define a second interval, thereby permitting a judgment of direction or length, one by the other. The more points added thereafter, the more real the line becomes. *In a total space, any number of separately visible points, irrespective of their distance from the observer, will form intervals which all lie in a plane apparently perpendicular to the direction of observation.* This fact can be verified any dark night when the stars are out. The stars all seem to lie in a curved plane perpendicular to the observer despite vast spatial differences along the third dimension. Presumably perspective and spatial judgment are impossible without a complete tridimensional reference system. The first and most fundamental activity of the visual process is the establishment of a coordinate system. *This is the field.*

Consider our straight line again, drawn between *three* objectively given positions and allowing two intervals. We still have disorder if the points are not equidistant, *i.e.*, if the intervals are not the same. However, if we take this disorderly unit of two unequal intervals, and add directly beneath it another pattern of identical configuration we have order. Though all four intervals are not equal, we do have two units of order, each unit composed of two *orderly* intervals. The result has a pleasing effect for the senses. The six dots have become welded into a whole, a complete pattern.

Any number of wholes, orderly or disorderly, may be repeated as units and go to make up still larger wholes. Since we made up an orderly whole out of two disorderly patterns, the order attribute (*Gestaltqualität*) was purely the result of the new positions of the points relative to each other. Thus, *new and different intervals result from the act of combination, and the relative directions of these newly created intervals somehow give order to the whole.* Since, *in vacuo*, direction is determined only by relative subjective position, lines drawn through objectively given equidistant points result in multidirectional parallel lines. The new intervals formed by the act of combination are *parallel.* This leads to our key definition:

Order is the term applied to any subjective quality or sensation which is produced by, and dependent upon, the number of straight lines which can be drawn through three or more actual or supplied points or centers of the sensory field; it varies directly with the degree to which these lines tend to become parallel with each other and with the vertico-horizontal coordinate system natural to the organism.

IV. Necessary Perceptual Constituents of All Experiences Exhibiting Order

Order is therefore dependent upon parallelism of lines. It is interesting to note at this point the conclusions reached by Ernst Mach in his inquiries into spatial physiological and psychological order. Mach began his investigation with the problem: why are not geometrically congruent figures always psychologically identical? He decided that: "The geometrical similarity of figures is determined by all their homologous lines being proportional, or by all their homologous angles being equal. But to appear optically similar, the figures must also be similarly situated, that is, *all their homologous lines must be parallel or, as we say, have the same direction.* By likeness of direction, accordingly, are determined like space-sensations, and these are characteristic of the physiologico-optical similarity of figures" (**6**, p. 109).

This statement of Mach's is italicized here because it it represents the closest approach to a systematic position on order heretofore reached by any investigator. Unfortunately, he failed to realize the importance of that one statement in his famous *Analysis.* On the other hand, it is significant that it was to this statement that he appended a footnote declaring: "Nevertheless, I am now as strongly convinced as I was then that the question involves the whole problem of form-vision." He was speaking of the ridicule he had once encountered in defending his views. This brief footnote is the only evidence that Mach ever appreciated the magnitude of the principle he had unwittingly stumbled onto. Apparently during and since his time no one else realized that it applies to infinitely more than the mere 'optical

similarity' of figures. It is nothing less than the secret or key to the whole psychology of man.

How shall we support this pretentious claim? Our interpretation must be subjected to a rigid test, a test so complete that it leaves no aspect of its validity unchallenged and unproved. We may start by observing that order is forever bound with form. Our method of presentation will follow that of Wertheimer's well-known dot figures in which the optical stimuli symbolize *all* stimuli, regardless of modality. Instead of taking a pattern composed of three points and giving two unequal intervals—the pattern used in the analysis above—let us construct our basic design of twice this number and even make the *directions* of all the resulting four intervals different. Figure I is just such a design. It is a

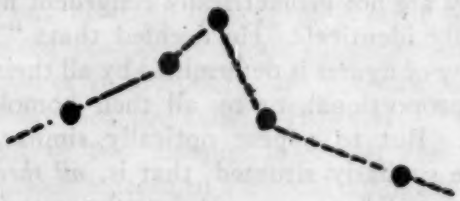


FIG. I. A disorderly design of five points.

group, cluster, or collection of five points. The broken lines drawn between them are merely to prove they have been placed so that their four intervals have neither a single direction nor distance in common.

In Fig. II we have taken this disorderly design of five points and four intervals, and using it as a basic pattern, have combined it with two other identical designs. Figure II is thus composed of three designs like that of Fig. I. It is a disorderly 'whole' made up of three disorderly patterns of five points each. Even though the three patterns are identical and though they lie in the same plane, the resulting 'whole' is a perceptual hodge-podge. In fact, we experience difficulty in dividing Fig. II into three equal parts, despite our acquaintance with the basic 'five-point' pattern. When we fill in the newly created intervals by drawing straight lines through all directions in which three or more of these points

are perfectly aligned, we can see immediately the cause of this disorder. *Only four or perhaps five such lines can be drawn. There are no parallels among them and not so much as a chance perpendicular.* Inasmuch as there are no parallels, there can

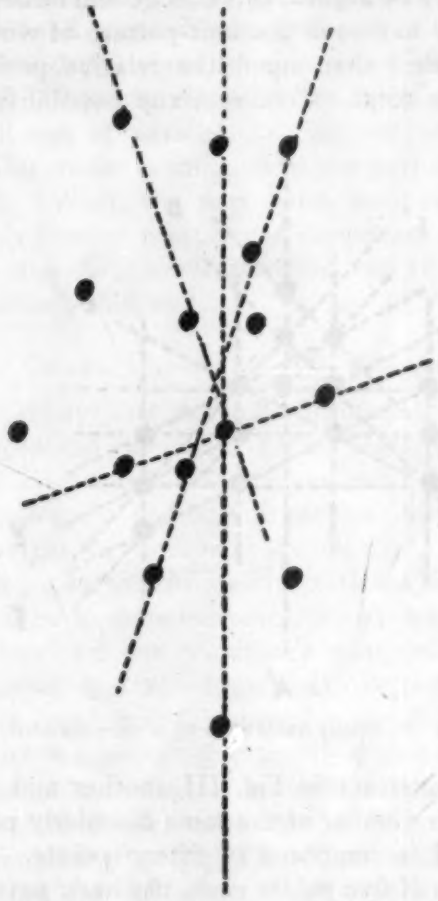


FIG. II. A disorderly 'whole' composed of three disorderly designs.

be no inner association or attraction of parts; ergo, there is no true order in Fig. II.

This figure indicates that *mere repetition as such of the same design does not necessitate order.* Such repetition itself must be orderly. We excluded the possibility of horizontal

or vertical order when the unit-patterns were constructed, and when they were combined together, their sequence was also purposely varied so as to eliminate any chance perpendiculars or parallels.

Still, it may be argued, this effect could be dependent upon the disorderly nature of the unit-pattern of which the whole was built rather than upon the relative positions of this pattern in the total. To dispel any possibility of this con-

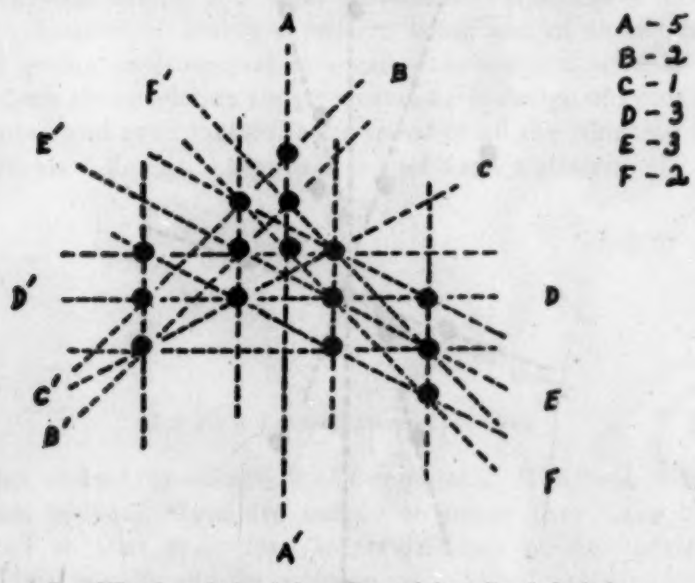


FIG. III. An orderly whole made up of three disorderly designs.

clusion, we construct in Fig. III another and *orderly* whole from the same number of the same disorderly patterns.

Figure III is composed of fifteen points. These are in three patterns of five points each, the basic pattern being the same as that used in Figs. I and II. However, they have been given different positions relative to each other and to the observer's eyes. They now have a great number of common intervals. Again we pencil through all directions in which three or more points are perfectly aligned. *We not only get sixteen such lines, but half of these lie in only two of the six directions represented—and these two directions are those of the*

vertico-horizontal coordinate system. All points but two have perpendiculars.

In comparing Figs. II and III, it is hard to believe that both were built of the same number of the same elementary designs in the same plane. The difference rests entirely upon the parallelism and perpendicularity found in III but absent from II. An orderly whole is constructed in terms of the distances and directions between its units. When these directions fall out of parallel, *i.e.*, do not have the same tangential value, order is confined to the part and leaves the whole chaotic. When the part itself, as in Fig. I, is disorderly, then all order whatsoever disappears as in Fig. II. *An orderly or disorderly whole, therefore, can be made of either orderly or disorderly designs.*[3]

V. Order Underlies All Behavior

Just what, if anything, does this principle of order have to do with perception, memory, habit, learning, and other psychological categories? What significance has it for systematic psychology? The organismic manifestations of this physico-geometric or morpho-psychological principle are extensive, but for an immediate illustration of its influence in lending coherence to mere existence, we have taken Fig. III, and drawn therefrom one of nature's most orderly constructions—a mineral crystal. Figure IV is merely Fig. III turned to the left until the direction *A–A'* is horizontal. Then, by filling in some of the intervals with a pen, and leaving others vacant, an effect of material solidity is obtained. Figure IV has exactly the same number of points in exactly the same arrangement as Fig. III. Certain directions have been emphasized at the expense of others. However, it must not be forgotten that this is only a two-dimensional drawing and consequently cannot reveal the myriad inner parallels which would necessarily occur in a tri-dimensional figure like a crystal.

[3] The contrast here made between order and disorder implies no strict dichotomy, but does stress whatever difference exists between better and poorer order. *Cf.* Wertheimer's opposition of good vs. bad Gestalten.

Glance at Figs. II and III and ask yourself which one you could and which one you could not easily reproduce if asked. From the informal classroom responses it appears that there is no question but that order is fundamental to memory and that recall is in proportion to it. As for visual

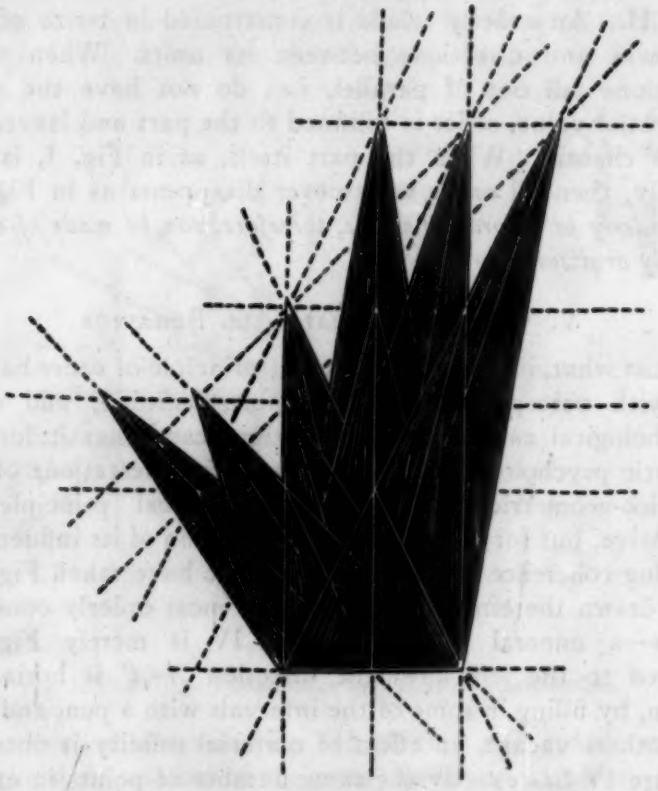


FIG. IV. A mineral crystal embodying exactly the same points and lines as Fig. III.

fields, a whole is a whole only if it has more order within itself than it has in relation to its surroundings; if the latter occurs, then the 'independent' or segregated whole ceases to be such or becomes a sub-whole in a larger whole. It disappears, fades into its surroundings, and cannot be seen. The use of camouflage demonstrates this figure-ground requirement perfectly. 'Meaning' is not a primary determiner

of visual or any other sensory wholes—for how can one attach meaning to something still unperceived?

There are many instances of order other than those of sheer spatial structure, such as the cases of order in society, in human institutions, music, the arts, customs, and habits. Let us turn again to our checkerboard. If we pass our eyes across it, we see a succession of squares in orderly sequence; we perceive a repetition, a *rhythm of squares*. Earlier we stated that order was a matter of regular repetition. Structural repetition results in parallelism, and functional repetition results in rhythm; one is spatial, and the other temporal; but both are order, and one and the same thing differently viewed. Just as the sense of sight depends largely upon spatial grouping, so the sense of hearing depends largely upon temporal grouping. Indeed, all qualities whatsoever depend upon grouping. Rhythm is in the heart beat, it is in the sex urge, it is the basis of skills, music, speech, dancing, and all the more elaborate social repetitions such as customs. *In a word, it is behavior* (**8**).

VI. Structural and Functional Order

There is another more difficult aspect of the order problem we must not overlook. Geometry and other forms of mathematics came into being as an expression of science; they are the language of physics because they convey the essence of the operations of the universe. So accurately do they express its activity that one does not need to be a Pythagorean mystic or an irrational rationalist to believe that our universe behaves *mathematically*. So inseparable are structure and function that one cannot possibly explain the action of an electromagnetic field without the use of spatial adjectives and nouns. It is equally impossible to explain the structure of the field without using the terminology of motion, *i.e.*, functional adjectives and verbs.

If we tell the mathematician that two or more forces meet, and ask him to calculate the resultant, he immediately wants to know two characteristics: first, the *intensity* of these forces with respect to an arbitrary *functional* standard;

and second, the *directions* in which they move with respect to a common structural *coordinate* system. Unless he knows both these factors, the functional and the geometric, he cannot possibly calculate the resultant. On the other hand, knowing these, he can compute, far more exactly than the physicist can measure, both the intensity and the direction of the resulting force. He does this simply by drawing figures which have structures complying with the geometric laws according to which these forces act.

The principle of continuity in projective geometry, first assumed by Kepler, then by Desargues, peculiarly exemplifies this inherent unity of structure and function. It states that the non-metrical characteristics which can be proved for a given geometric figure will hold true if that figure should change its shape in any manner, subject to the principles or conditions according to which it was first constructed. Properties which can be demonstrated for a given figure are directly dependent upon the rules under which the figure was produced, *i.e.*, the initial *order* involved.

These principles of order underlying all mathematics are also the basic laws underlying all natural processes. Just why should the shortest distance between two points be a straight line if this same straight line were not the path of least resistance taken by any force through a constant medium? Why should the perpendicular be the shortest distance from a plane to a point if this perpendicular were not the path of least resistance from any plane or field of equal tension? If these latter physical conditions were not the natural and normal conditions of our universe, then the theorems of geometry would no longer be applicable to the structural circumstances of this earth, for it was from the observation of these structural or spatial conditions that geometry was born and its propositions formulated. The existence of these similarities and uniformities is foundational to all logical interpretations of the nature of man and the world.

Why should all the natural sciences display so close a correlation between the structural and the functional? Simply because everything that exists moves, and everything

that moves describes a path with respect to a given coordinate system. It is a vector. The shape of this path is determined by the geometric laws of the inner forces which determine the movement plus the outer conditions under which it takes place. Whether it is the space-time continuum which is geometrically constant with respect to the forces, or conversely, is a point too intricate to develop here. The important fact is that this constancy exists. Whenever a thing functions, the forces determining the function will also determine a configuration. This configuration can be developed by mapping the paths of movement when these are known and not too complex, and such a 'map' will correspond to the map of any other identical function irrespective of location. On this principle all physicists agree. One may then state with reasonable certainty that *all identical movements and functions, whatever their intrinsic nature, determine identical configurations which are subject to the geometric laws inherent in the existence of the forces involved.*

VII. Concluding Argument

It follows from the last statement that *if psychological action is neural action and this in turn is admittedly electrical or electrochemical action, then they all ultimately display configurations subject to the peculiar geometric laws of electrical processes.* If our mental and visual images are somehow electrical structures or 'designs,' they are built in accordance with the laws of electricity; and these laws are known and can be defined. Moreover, if mental activity is electrical, the mind should be better able to grasp and retain those figures which have structures complying with the geometry of electricity. These figures are easily drawn, for this geometry is familiar.

The above statements do not necessarily mean that when we visualize 'vase' or 'dog' some sort of 'psychological lightning' will streak out of a subcortical area and describe the contours of a vase or a dog in our sensory space. It is not so simple as that. Yet we suspect such an interpretation to be really more true than false. There must be certain struc-

tural regularities or *geometric constants* in the dynamic configurations of the processes involved in the organism as a whole, whether it sees, thinks, or remembers. Regardless of the nature of the psychological action, these constants will not and cannot vary in any intrinsic way.

Significantly, the 'geometry' of electricity is unique. Thus, a current sets up an electromagnetic field *perpendicular* to its direction of flow. This field acts in a direction *perpendicular* to any diameter. Electricity is the only force where two *parallel* vectors having the same direction of flow demonstrate an attraction for each other. Every electric current sets up a surrounding electromagnetic field with directions of action identical with those of a perfectly standard Cartesian coordinate system. This current attracts or repels other currents in proportion to the parallelism between them.

The process of identification is now complete. *If we go back to our original analysis of structural order, we find that the coordinate system and inter-attraction brought about by parallelism and perpendicularity are the very characteristics whereby psychological order is defined.*[4] This is the basic principle of psychological action. All such behavior is electrical in character and involves electromagnetic fields.

BIBLIOGRAPHY

1. Carr, H. The quest for constants. Psychol. Rev., 1933, 40, 514–532.
2. Dewey, J. *The quest for certainty.* New York: Minton, Balch, & Co., 1929. Chapter IX, 'The supremacy of method.'
3. Ehrenfels, C. von. Ueber Gestaltqualitäten. *Vierteljahrschrift für wissenschaftliche Philosophie*, 1890.
4. Koffka, K. *The principles of Gestalt psychology.* New York: Harcourt Brace & Co., 1935.
5. Köhler, W. *Dynamics in psychology.* New York: Liveright Publishing Corp., 1940.
6. Mach, E. *The analysis of sensations.* Trans. by C. M. Williams. (5th ed.) Chicago & London: The Open Court Publishing Co., 1914.
7. Nafe, J. P. The pressure, pain, and temperature senses. *In* Murchison, C., *The Handbook of General Experimental Psychology.* Worcester, Mass.: Clark University Press, 1934. Pp. 1037–1087.

[4] Note that this argument is a special application of the principle that things 'equal' to the same thing are equal to each other. If the conclusion to which this argument leads is sound, the doctrine of isomorphism has acquired a new and more solid foundation. Field theories in general should also be better established as a consequence.

8. RUCKMICK, C. A bibliography of rhythm. *Amer. J. Psychol.*, 1913, 24, 508–519; 1918, 29, 214–218; 1924, 35, 407–413.
9. RUSSELL, B. *The a.b.c. of relativity*. London & New York: Harper & Brothers, 1925.
10. SICKLES, W. R. Experimental evidence for the electrical character of visual fields —derived from a quantitative analysis of the Ponzo illusion. *J. exp. Psychol.* 1942, 30, 84–91.
11. SPINOZA, B. DE. *Ethics*. Trans. by A. Boyle. New York: E. P. Dutton & Co., 1910. Part I, Appendix.
12. VERNON, M. D. *Visual perception*. London: Cambridge University Press, 1937.

[MS. received March 9, 1942]

AN APPROACH TO THE STUDY OF THE SOCIAL ACT

BY MAPHEUS SMITH

University of Kansas

A variety of conceptions of the unit of activity has been suggested as general elements in behavior,[1] all of which may be subsumed under the following definition: In its broadest terms an act is an observable alteration of a living organism that can for convenience be differentiated from other acts and can be said to change through a phase of relative disequilibrium of the energy-system of the organism from a beginning phase of relative equilibrium to a final phase of relative equilibrium. The social act may similarly be defined in terms of larger and more complexly interrelated energy-systems consisting of the same phases.

The simplest variety of conceptions of the unit of action employing neuro-muscular structure and function is the reflex act, consisting of transmission of impulse from an activated sense receptor (in the retina of eye, in the skin, in the inner ear, in the patellar tendon, etc.) over the structural gap between the receptor cell and a motor cell, and thence to muscles which, in contracting, produce a movement, such as an accommodation reflex, a startle reflex, or a knee jerk. No intermediate structures and no other functional phenomena are required to explain the muscular contraction of such a simple act,[2] and no significant time interval occurs between the beginning and end of the action. Awareness by the individual is also unnecessary and actually does not intervene between impulse and con-

[1] For example, see Ellwood Faris, The retrospective act, *J. educ. Sociol.*, 1940, 14, 79–91; T. D. Eliot, Reactions to predictive assumptions, *Amer. sociol. Rev.*, 1937, 2, 508–517; and G. H. Mead, *The philosophy of the act*, Chicago: Univ. of Chicago Press, 1938.

[2] It is significant that in many reflex acts the whole process is carried out without recourse to the higher nervous centers, being consummated through actions at the level of the lower spinal cord. The patellar reflex is the most commonly known example.

traction. Conscious discrimination, judgment, decision and self-control have no part in such a simple form of activity.

For a long time the reflex arc conception was considered to be a satisfactory unit for analyzing behavior. And it is still true that the sensory and motor parts of all actions are recognized to be of the greatest importance in interpretation. But the reflex conception is now generally discarded as a basis for a dynamic psychology or social psychology, because of its underemphasis on the intervening portions of the complex forms of action, which occur between the sensory and motor phases.[3] Whenever a combination of impulses and muscle groups, even as few as two distinctly different ones of either and one of the other, the simple reflex concept is inadequate. It fails, because it is too elementary to be a realistic interpretation, and is too simple for general applicability. Almost every instance of action of vertebrate animals requires a more complex scheme of analysis, marked by coordination of either sensations or movements, generally both. And, as we approach the analysis of human behavior in social situations, more and more elementary sensations, intermediate contributions and muscular contractions must be taken into account. Among the most important added items is some means of storing up experiences from past behavior and bringing them to bear on present sensory situations. The maintenance of a sensation or memory image during a period of coordination or 'solution' of a problem of adjustment [4] is a still further advance, and is confined to only a few of the higher mammals. Of as much advantage is the ability to hold muscular movements in abeyance while coordination or problem solving takes place. The highest forms of each of these abilities and processes are found among the most intelligent and self-controlled human beings.

It appears from a consideration of all of the various sorts of activity that are theoretically distinguishable in a series ex-

[3] *Cf.* Dewey, The reflex arc concept in psychology, PSYCHOL. REV., 1896, 3, 357–370, for an early criticism. Also see E. B. Holt, *The Freudian wish*, New York: Henry Holt, 1915; and E. C. Tolman, *Purposive behavior in animals and men*, New York: Appleton-Century, 1932, Chapter I.

[4] How this happens is not fully understood. An attempt is made by E. B. Holt to explain it in complex mechanistic terms by use of the concept of *sustained reflexes*. Cf. E. B. Holt, *Animal drive and the learning process*, New York: Henry Holt, 1931.

tending from reflex acts to the most conscious, deliberate and purposeful of actions that a great deal of further study should be devoted to the forms of action. It also follows that, in order properly to study each sort of behavior, a different construct will be required, each entirely adequate for its task, but none entirely suited to any of the other tasks. Therefore, for each sort of behavior it will be necessary to construct an explanatory device which will be realistic, generally applicable to that class of actions, and incapable of further subdivision.[5]

The elaboration of analytical constructs for all of the significant forms of action is obviously impossible here. For our present purpose we shall make a short cut and consider only the sort of act that is suitable for understanding the most complex individual behavior and at least the simplest form of social action. After this is done we shall examine the possibility of applying the same analysis of the individual act to social actions of a more complex sort.

The special characteristics of conscious, anticipatory activities directed toward clearly defined goals, which are the most highly organized actions of which we have any knowledge, are the complexity of the circumstances, the complexity of the activity that occurs, the fact that the behavior is not automatic but requires perception and intelligence, and the fact that partial adjustive alterations, which we may designate *instrumental activities*, are necessary before the goal may be reached. Such acts begin with relative equilibrium and include (1) an *impulse* arising from an alteration in the relationship of the individual to the environment, (2) *perception* of disequilibrium, a goal and a means of approaching the goal (which will lead to satisfaction, or equilibrium again), (3) activity toward the goal in which, if necessary, a variation of alterations (*manipulations*) will be employed up to the limits of possible variation of the organism's behavior under the circumstances, and (4) *attainment* of the goal. The last stage leads in turn to relative equilibrium of the organism-in-environment system.

[5] *Cf.* Mapheus Smith, The greatest common component in science, *Phil. Sci.*, 1942, 9, 30–36, for discussion of the most desirable element in scientific explanations.

Of these four aspects of complex action the impulse and attainment phases are not restricted to such acts, since they merely refer to the stimuli which upset the equilibrium of the organism, and a return to equilibrium, which are phases of reflex actions. Attainment, however, may be defined in terms referring only to goal-directed activity, in which case only impulse is common to all actions.[6] Perception is specially important for complex behavior. The simplest and most automatic responses do not require this step. Indeed, all parts of the behavior sequence between impulse and return to equilibrium are absent in the reflex act. But when the goal to be reached is large, complicated, or not immediately present to the senses, perception is necessary, as is variation in behavior or sustained action while the intervening alterations (means of reaching an end) occur. The perceptive and manipulative phases of behavior do not require wholly new interpretative conceptions. Perception in the restricted sense here used may be best understood as the process of organizing the images of the manipulatory phases of past activities. This process is not fully explained by psychologists yet, and is not subject to complete observation, but in simple terms past manipulative actions and experiences of those actions are combined in the shortest and sketchiest form suitable for fulfilling the function of adjustment, or solution of a problem. The manipulative phases may be numerous in each behavior unit. But each may be thought of as an act of a psycho-somatic character, since there are psychic and muscular-glandular phases of each. In this sense each exhibits energy release (alteration), relative equilibrium and relative disequilibrium, and is started by a stimulus. Thus, the whole act is a system made up of interrelated parts, each of which is functionally related to the situation and the goal-directed action, but each of which exhibits the elements of equilibrium, energy release and return to equilibrium.

The outline of four phases of the complex act given above,

[6] Of course, equilibrium and disequilibrium are always found in reflex action, as well as are sensation, neural transmission and muscular contraction.

derived in part from the writings of G. H. Mead,[7] is not to be conceived of as invariably showing this sequence. The only fixed order of events is that of relative equilibrium, impulse (relative disequilibrium), alteration of the acting unit, return to relative equilibrium. Strong impulses are needed for manipulative behavior, but mild ones often lead to perception and to thought or muscular manipulation before the specific impulse producing goal-directed behavior takes place. Manipulation also may precede clear perception of a goal. Perception and impulse may occur at any time in the behavior unit and are almost constant factors, as is true of impulse. But in spite of variations the necessity of the four parts is inescapable, and in a considerable proportion of cases the logical sequence is followed.

The adequacy, however, of this analysis of complex behavior must be carefully tested against the four criteria of components suitable for scientific generalizations. As we have seen, it is generally applicable to the most complex activities of the individual, although the same sequence of items is not found in all cases. It probably cannot be shown to be universally applicable to all non-reflex actions, however, and may be further complicated in the more complex forms of social behavior. For example, if the person has, through practice, developed a stereotyped sequence of activity, the details of which do not require 'manipulation,' some of these intermediate phases of action will not require the four-part behavior unit for their interpretation. But it still is generally satisfactory for the unstereotyped, voluntary and intelligent type of adjustment to a situation.

There is no difficulty with the second criterion, since the parts of the unit are not further divisible into common elements of any great significance. Each part is as elementary as it is necessary to make it. The conception also fulfills the desideratum of articulation to reality. Each person's self-observation will reveal to him a variety of impulses, material objects and other perceived items, thoughts (mental manipula-

[7] Mead, *op. cit.* Also see Kimball Young, *An introductory sociology*, New York: American Book Company, 1934, pp. 78–84.

tions), muscular manipulations, and the satisfaction of attainment. Empirical reality in the social sense is not demonstrable for all of these items. It is true that impulse may be defined in observational terms by distinguishing alteration in the organism in reference to observable outside events, but the occurrence of perception must be inferred from the quality of the observed behavior, or from a comparison with what behavior could occur in the complete absence of perception. The observer's own perception must be the basis of his inference of another's perception under each set of circumstances. Physical manipulation can be directly observed, but the manipulation of thought or insight must be inferred, either from its necessity for explaining a line of action or from language or other overt expression. Attainment may in some cases be observed as locomotion toward a location, the arrival at which leads to quiescence. But in other cases it must be inferred aside from locomotion, entirely in terms of quiescense following relatively intense action. It is thus clear that most of the components of the complex act accord with observable reality in many cases. However, the fact that another person uses perception must always be inferred and in many cases other parts of the act must be inferred. On the other hand, the necessity of inference and self-observation does not imply the lack of orientation to reality, but rather the essential reality of the interpretation for human beings. People do make self-observations and their intelligent, goal-directed acts are not capable of adequate interpretation except by the use of a subjective scheme of the sort in question.

There can be no serious doubt about agreement with the fourth criterion, the use of as large and rich a conception compatible with the other criteria. The perceptive and manipulative parts of the act allow for the richness of emotional perceptions, as well as perceptions of material objects of the present, images of the past or future or of an ideal world. Manipulation also may include the whole repertory of reason, and imagination, on one hand, and muscular manipulation, on the other. Impulse and attainment may also be on a very extensive plane. In every respect the extreme richness of the

most complete and complicated personality is capable of interpretation by the proposed scheme.

A science of social behavior, like a science of individual action, requires a dynamic approach. And, if it is to be a generalizing science, it is in need of a satisfactory conception of the unit of social behavior: the social act. When the notion of social as associative is combined with action as a process of an agent, *social action may be defined as change involving an agent in a social environment which he influences or is influenced by*.[8] This broad conception needs to be considerably qualified in practice, since it includes all sorts of marginal social actions as well as interactive behavior. We should specify animate organisms as the agents, if we are to separate physics and chemistry from other sciences dealing with objects in various collective relationships. And we are justified in further restricting attention to 'free-moving' living agents: members of the animal kingdom. Whether social scientists are justified in making sharp distinctions between the interaction of insects with each other, of dogs with each other, of sheep with each other, of anthropoid apes with each other, on the one hand, and of human beings in social situations, on the other hand, and to accord the term social only to the actions together of human beings, is not so readily decided.[9] There is as much value in a broad definition of social agent and a designation of different classes of agents and corresponding social actions, such as those of animals and those of humans, as to define social on such a high level that only human beings, and possibly not all of them, can be demonstrated to engage in social behavior. Whatever the terminology employed, however, it is clear that all actions of organisms in social situations are not exactly alike, and it is therefore necessary to distinguish more

[8] *Cf.* E. E. Eubank, *The concepts of sociology*, Boston: D. C. Heath, 1932, p. 285, for a somewhat similar view, called *transitive action*.

[9] For the humanistic view of social the following are representative: F. Znaniecki, *Social actions*, New York: Farrar & Rinehart, 1936, Chap. I; C. H. Cooley, *Human nature and the social order*, New York: Scribner's Sons, 1902; and E. Faris, *The nature of human nature*, New York: McGraw-Hill, 1937, Essay I. In contrast, see C. Murchison (ed.), *A handbook of social psychology*, Worcester: Clark Univ. Press, 1935, in which a large part of the subject is on the animal level; O. Klineberg, *Social psychology*, New York: Henry Holt, 1940, Chap. II; and W. C. Allee, *The social life of animals*, New York: Norton, 1938.

than one set of characteristics of associated action and more than one conception of the social act.

The primary distinction to be employed at each level of interpretation of social action is that between unilateral[10] and reciprocal social action.[11] The former consists of actions in which no participant both stimulates and responds to the other participants, while in the latter case each participant is responded to and in turn responds. Examples of unilateral social action have in common the behavior of one participant, A, with reference to some social stimulus (either person or artifact), but a failure of the stimulus object to act with reference to A. 'Acting with reference to' means acting with 'perception of' and either for the purpose of adjusting to B's characteristics, of influencing B in some way, or a combination of the two. A policeman may see and approach a criminal without the criminal being aware of the fact. Or the criminal may see the policeman and hide or silently slip away.

When an agent attempts to influence another agent who is able to become aware of the action of the first, reciprocal activity will naturally follow unless the action is arbitrarily analyzed into two unilateral parts of a bilateral act. But on many occasions a person is indirectly affected by the acts of those he does not see or know. A rich man may leave a legacy to a person yet unborn. A jilted lover writes a letter, posts it and then commits suicide or is killed in a traffic accident. In modern society, where indirect contacts are tremendously multiplied, unilateral forms of social action have become very numerous. The process of administering a large organization where orders given by an executive proceed by means of memoranda through a series of one-way relationships to the ultimate worker, who has no direct contact with the executive, is almost universal in economic, political and military life. Only a strongly developed tendency to execute orders without question enables such an organization to function.

[10] *Cf.* the concept of *linear* social behavior in F. H. Allport, *Social psychology*, Boston: Houghton Mifflin, 1924, pp. 148–149; and Eubank, *op. cit.*, p. 285 (*non-reciprocal* action).

[11] *Cf.* Allport, *op. cit.*, pp. 148–149; Eubank, *op. cit.*, p. 285; and R. T. Lapiere, and P. R. Farnsworth, *Social psychology*, New York: McGraw-Hill, 1936, pp. 365–366.

In simplest terms the unilateral social act may be assumed to be preceded by relative equilibrium in the relationships between an agent and his social environment. Some alteration of social relationship functions as an impulse to one participant. The alteration is perceived in relation to the original circumstances and also in relation to the goal of the perceiver. Means of acting with reference to the changed circumstances are also perceived. Manipulations are employed and the perceiver attains his goal, but this goal does not include the action of the stimulator with reference to the perceiver and responder. There is no important difference between such an act and the goal-directed action of an individual, except that the stimulus pattern includes another element, either organism or material object (such as a cultural artifact), which makes no response to the active agent.

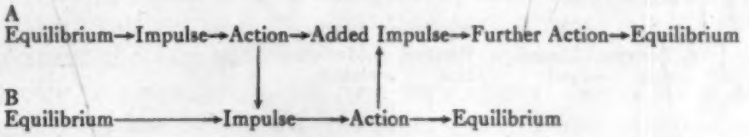
As described, linear social action cannot be explained by reflexes alone. It is possible for one person, unaware of the presence of another, to flash a bright light which brings forth the accommodation reflex in the second person. But it would not be possible to locate two people so that one's reflex response would arouse the other's reflex response, which, in turn, would then reactivate the first, completing a stimulus and response cycle on a purely reflex basis. And even in the case of the linear reflex response, the action of the person with the light cannot be described as a reflex. No action 'with reference to' another, engaged in by any person who looks at or is attentive to another, is merely a reflex. Perception and adjustment are prerequisite and these are 'higher level' actions.

The simplest circular social response is that in which each of two organisms functions as a stimulus to the other and acts with reference to the other.[12] The initiator of the action, A, may not act with reference to the other, B, but his action arouses B, who acts with reference to A and also in such a way as to call A's attention to the relationship between them. So long as this is done, even should A only become aware of the

[12] *Cf.* E. D. Chapple, Quantitative analysis of the interaction of individuals, *Proc. Nat. Acad. Sci., Wash.*, 1939, 25, 58–67, for a method of analyzing reciprocal social behavior.

relationship after B is no longer in contact with him, the action has been bilateral, although on an implicit basis. When A's recognition of the relationship is put on an overt basis, the action is then explicitly complete. This may be done by any muscular movement, or break in the action A is carrying out, but is usually indicated by a clearly meaningful response or reply on the part of A, the full variety and extended character of which is of minor significance for our present purposes.

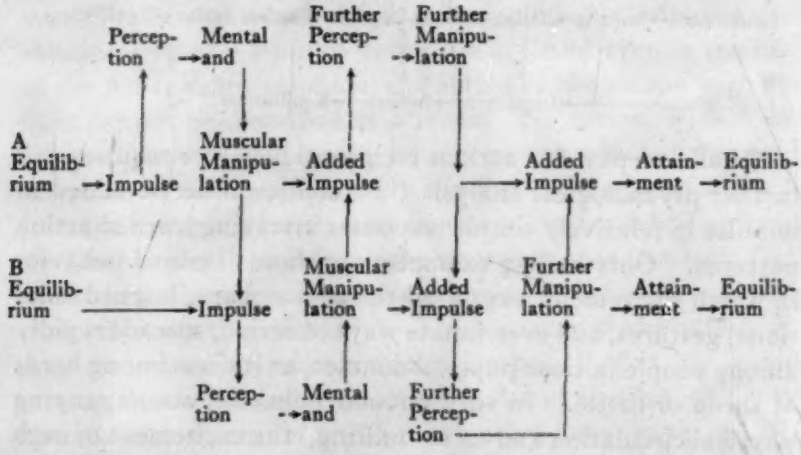
The unit of action in reciprocal behavior begins with relative equilibrium which in the case of participant A, who initiates the social act, is upset by some impulse. This event occurs prior to and is an impulse in disequilibrating the second participant, B, whose response acts as a further impulse in A's behavior. The accompanying diagram graphically represents the simplest two-sided social act. In acts of longer duration, other interchanges occur with further impulse, reaction and interaction elements being added to the minimum represented in the diagram. Other complexities develop from the addition of other participants who are related in various ways and who participate directly and indirectly in social behavior.



In all except reflex actions reciprocal behavior requires still further psychological analysis. Perception must be added to impulse in relatively simple responses involving learned action patterns. Outstanding examples are those of crowd behavior in which excitement, expressed through slogans, learned emotional gestures, and even innate ways of acting, spreads rapidly among people in close physical contact, as it does among herds of sheep or cattle. In such circular behavior, accompanying physical circulation known as 'milling,' the excitement of each organism adds to the excitement of every other and is reinforced in its turn by the increased activities of others. Such action rests on perception, but to only a very slight degree on

interpretative levels of response.[13] In place of B's response to A being different from A's action, by the extent of B's interpretation of A's act, it is merely a reproduction, or, in some cases, the release of complementary expressions, with heightened expressiveness. Impulse, perception and immediate reproductive response are characteristic of each person's action under these conditions. But intentions, manipulation and attainment are not clearly demonstrated.

In interpretative and symbolic social behavior the manipulation and attainment phases of the act are accompanied by the higher forms of perception. The characteristic social act on the purposive level of response consists of interaction between extended and psychologically complex actions of two or more people. The next diagram represents such an act. This example involves both thought and physical manipulation by both participants in order to attain their goals. Perception involves goal perception, perception of the external situation, of the self and other items. Each manipulation, since partially muscular, functions as a further impulse to the active organism, as well as to the other person, leading finally to attainment for both participants and to final equilibrium.



Such an analysis is applicable to the two main varieties of reciprocal action: communicative behavior and socially con-

[13] *Cf.* Herbert Blumer, in R. E. Park (Ed.), *An outline of the principles of sociology*, New York: Barnes & Noble, 1939, p. 225.

trolled behavior. In communication the muscular activity of each participant, which acts as an impulse for another participant, conveys some meaning, namely, is understood by the other. It is possible for a small amount of common understanding to develop on a unilateral basis, as when one person 'reads another's mind,' that is to say, perceives, by means of inference, the perceptions (knowledge, feelings, attitudes or desires) of another person, but is not similarly understood by the other. Such behavior is rare, however, and relatively unimportant. Reciprocal understanding behavior, marked by each participant 'reading the mind' of the other, is practically universal and is indispensable for human society. But unless each participant makes some overt expression which another can interpret, and unless each interprets the other's overt expression there is little likelihood that each will understand the other. And unless each can understand the other's experiences, harmony cannot be recognized to exist, nor can it be brought about with certainty, or maintained or repeated, all of which are necessary for organized group life.

In each reciprocal communicative act the intention of each participant is to understand and to be understood. In order to attain this goal it is essential that each express himself and interpret the expressions of the other, made *after*, and supposedly in response to, his own expression. Not until each participant has concluded this process to his own satisfaction is the goal of each attained and the act completed. For illustrative purposes we may take an example from the commercial world. In a motor car agency with fixed but unmarked prices a potential customer desires to learn the price of a car with certain accessories, and the salesman desires to have the price and the qualifications of the car understood in such a light that the understanding will help to bring about a sale. Some communicative actions motivated by such desires begin with the customer making the approach and asking for the complete information. The salesman receives this request, perceives its meaning, and provides the information. The customer repeats the information, and is assured by the salesman that his understanding is correct. The salesman understands the

customer's desires as soon as he hears the request, thus satisfying his first desire: to understand the customer, as a step to the consummation of a sale. At this moment, and before the salesman's reply begins, the customer has been understood, which is one of his goals, but he is not aware of having been understood and he understands nothing about the price at that moment. The reply of the salesman evidences his understanding, and when it is comprehended by the customer, both of the desires of the customer are realized: he now understands the price and the kind of car priced, and can realize he has been understood. The salesman, however, cannot be certain he is understood until some further response is made by the customer to signify the interpretation the customer has made. When he hears and comprehends that the customer has understood correctly, the goals of both participants to understand and to be understood have been attained. The minimum essentials of this particular act are then complete. But the customer may not yet be certain he understands and he may, by means of inflection, a hesitating manner, an inquiring look, or a specific request, convey his desire for further assurance. The restatement by the customer not only shows the salesman that he is understood, but it is also a stimulus to reassurance by the salesman. The salesman's second response is certification to the customer. If the customer does not indicate his understanding by reproducing what he was first told by the salesman, but replies ambiguously, as by a nod and a "thank you," or by "yes, I understand," the salesman will be forced to ask and receive a reply that will assure him that he has been understood. This he may not do, but since such a communicative act is ordinarily but a step in the direction of influencing another person to act cooperatively, we may be sure that in most cases some certification of understanding will take place, and that persuasion or other methods of influence will be employed.

What has just been stated concerning reciprocal acts applies to every communicative act, since each depends on the employment by each participant of some overt behavior that is interpreted in the same way or a similar way by the person

who expresses it and another person who interprets it. Each such item of expression symbolizes, represents, is a substitute for some perception or experience. But the symbolic aspect of the communicative act is no more important than the perceptive process. Indeed, as indicated above, each intentional communicative act involves interaction of impulse, perception, manipulation (part of which is overt and symbolic), and attainment, carried on by two or more people making adjustments to each other. Omission of any of these items makes the complete social act incomprehensible.

The view expressed here is in agreement with that of Bentley, in his use of the concept of *communicane*,[14] which refers to 'men seen in conversation about something.' The communicane, if we extend the content of *dicaudane* (the speaker, hearer, air, and the embodied reference of the vocal conversational remark) [15] to general cases of communicative activity, would include a communicating person, his communicative movements sensible to others, the medium by which they arrive at the sense organs of another person, the communicant, and the reference of the movements. But when we specify the total action of the communicating person and the communicant, including both their movements and the necessarily inferred items of impulse, perception, intent, mental manipulation, and selection of movements to employ in communicating the perceptions, all of which must be included for satisfactory explanation, the analysis is no less accurate, and considerably richer in content. And when we specify the beginning of the fully communicative act as the impulse of an agent to communicate, and its end as certified mutual understanding and being understood, we have clearly championed a larger and more valuable unit of action which is completely justified by a principle of defining scientific constructs according to problems dealt with.[16]

It is necessary to go one step further in the analysis of

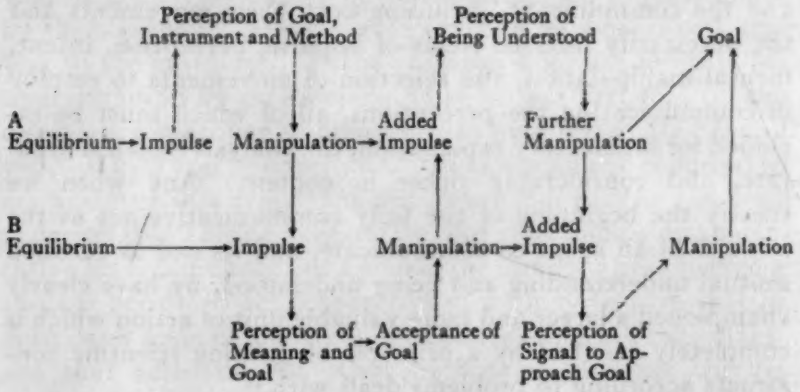
[14] A. F. Bentley, *Behavior, knowledge, fact*, Indianapolis: Principia Press, 1935, p. 251.

[15] *Ibid.*, p. 233.

[16] See Bentley, *op. cit.*, pp. 210–211; and J. F. Brown, *Psychology and the social order*, New York: McGraw-Hill, 1936, Part I.

social action. As important as communication is for human society, and as prerequisite as it undoubtedly is for social order and social organization, it is not adequate to explain them. Unless we go on to controlled action our analysis remains on a casual, impermanent and immediate social level. As Znaniecki has adequately demonstrated, the most important social actions involve an agent, employing social instruments, by means of social methods, so as to obtain a response desired by the agent.[17] Although it is not justifiable to eliminate communicative acts from the category of social action, it is necessary to emphasize the greater significance of control.

Most socially controlled behavior is merely a slight extension of communicative activity. If control is given the broad meaning of directive influence, whereby one person's action is willingly altered to accord with the intention of another, we have to add the item of a goal on the part of the controller to influence a respondent to help the controller accomplish some specific thing. The controller also must have the contributing goal of being understood. The respondent must have the ultimate goal of cooperation, and this goal also is dependent on communication. But the controlled action



proceeds at least one step beyond understanding to a further overt goal-directed accomplishment toward some material object. The accompanying diagram illustrates such an act. A wants B to help him by judging the quality of a book that A

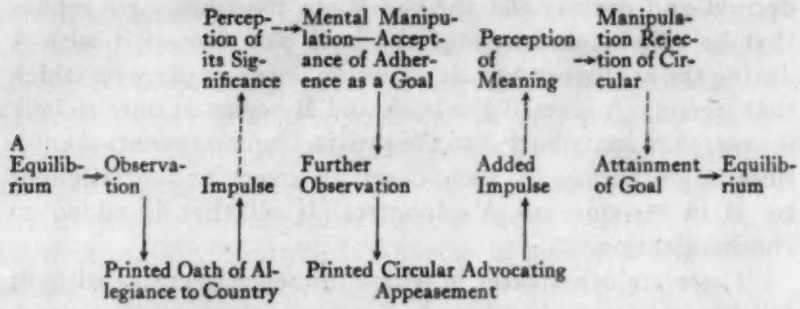
[17] F. Znaniecki, *Social actions*, New York: Farrar & Rinehart, 1936, pp. 71–77.

is expected to review for a newspaper. A next perceives the form of appeal that will be likely to have the desired effect on B. Then he explains what he wants and makes the request, possibly both in a single statement. B understands what is desired and decides that he can spare the time. He replies that he will be glad to read the book and discuss it with A during the next week end, at a specific time and place on which they agree. A gives B the book and B begins at once to look it over, thus contributing to the satisfaction and eventual equilibrium of both. In such cases an overt accomplishment by B in response to A's 'control' is all that is added to communication.

There are other cases in which influence operates without full consciousness, as when A uses suggestion or a command, and B carries out the implications of the suggestion or command without questioning its value, without sympathy, or without understanding its full implications. But there is some communication here, and even hypnotic suggestion rests on understanding without full awareness of the total psychological situation of the witness or the hypnotist.

The analysis is not applicable to all societal control, however. When a person is influenced by laws, customs and moral codes, but not by immediate activities of others, his behavior is restricted and in a sense controlled, but not in an immediate or fully social sense. The action here is clearly one-sided and incomplete, and depends on the individual's reaction to his knowledge and to artifacts of the culture, rather than to the expressed goal-directed instruments and methods of influence of an active agent. Such behavior is as much societally controlled, and more self-controlled than it is socially controlled. The next diagram, compared with the previous one, will illustrate the essential differences between societal and self-controlled action, on one hand, and socially controlled action, on the other. Except for the fact that the agent's impulses are contributed to by artifacts, the act is like that of any other individual in response to an environmental stimulus. The control element is entirely dependent on the person's past experience, his conception of how other people react, the de-

cision he comes to as a result of this combination, and the self-control he is able to employ in responding to stimuli contradictory to his decision and perception. These elements are found in examples of personal and immediate social control,



but in the person-artifact situation just described the dynamic personal interplay is completely absent, just as is reciprocal activity and bilateral communication.

The present analysis has indicated that all social action is psychologically complex, even when we arbitrarily restrict the discussion to the simplest sorts of human relations. The application of the same sort of approach to more complex social behavior will necessarily be tedious in proportion to the complexity, but this fact is not a valid argument for simplifying the conceptions to be employed. In crowd behavior, as well as in organized group behavior, the elements of relative equilibrium and relative disequilibrium are found applicable to every act. Impulse is always a factor, and no social action transpires without perception of some sort, however excited or vague it may be in some collective situations and for some people. Manipulations, both mental and muscular, are also characteristic of complex actions, as are meaning, decision concerning goals and attainment. Certification of communication is also found, and social influence is found in a variety of complex relationships of people. The complete psychological analysis of all of the various forms of complex action remains to be made, although recent efforts have made considerable progress.[18]

[18] *Cf.* especially L. von Wiese, *Systematic sociology*, New York: John Wiley, 1932, pp. 230 ff.; Znaniecki, *op. cit.*; and R. T. Lapiere, *Collective behavior*, New York: McGraw-Hill, 1938.

The most complex social act, however, exhibits some important characteristics not found in the simpler actions. Many people do not fully participate in larger social situations. One-sided relationships and actions are common.[19] Incomplete understanding is probably frequent, as is uncertified communication. Unilateral control is often present. In large groups many people are relatively unimportant, such as bystanders, camp followers, spectators. They are incompletely stimulated, incomplete in response, lacking in purposes or tangible accomplishments, apparently making little or no contribution, having little influence on others, frequently appearing to stimulate or influence nobody. In other words, the relationships and actions of people in complex situations are far from equal and they differ in many important ways, both overt and covert. Nevertheless, any purposive behavior of a social agent, whether an individual or a collectivity, requires perception, manipulation and attainment, in addition to impulse, and can be interpreted in terms of relative equilibrium and disequilibrium. And even when a group seems merely to 'muddle through' a situation that confronts it, we still find the same functional elements in operation. Perception and manipulation are found along with impulse, even in what we think of as aimless behavior, which continues until the temporary equilibrium of fatigue or the permanent cycle of collapse terminates the sequence. When there appears to be no clear group unity or purpose problems still may be apprehended and may impel people to develop consensus of ideas and objectives and to carry out a sequence that restores equilibrium by meeting the problem. Even in larger groups, such as a government, a business corporation, or an army, the same action sequence is found as in simple interaction. Some people must perceive problems and possible solutions. They must be capable of manipulating ideas and planning the coordinated

[19] *Cf.* J. L. Moreno, Who shall survive? A new approach to the problem of human interrelations, Nerv. ment. Dis. Monogr. Series, No. 58, 1934; G. A. Lundberg & M. Lawsing, The sociography of some community relations, *Amer. sociol. Rev.*, 1937, 2, 318–335; G. A. Lundberg & M. Steele, Social attraction-patterns in a village, *Sociometry*, 1938, 1, 375–419; and J. H. Kolb & E. deS. Brunner, *A study of rural society* (2nd ed.), Boston: Houghton, Mifflin, 1940, pp. 156–159.

activities of the whole or a representative part of the whole. The plan and procedure must be transmitted throughout the integrated part of the group so that the manipulative functions may be adequately consummated and the problem solved. Larger social systems are never completely efficient in their operations, but all of them appear to exhibit the basic functions we have discussed.

It therefore seems possible to conclude that this scheme is as widely applicable as any that has hitherto been elaborated for social behavior of the conscious and intentional sort, and that we may have in the conception of impulse-perception-manipulation-attainment, beginning in relative equilibrium, and proceeding through relative disequilibrium to relative equilibrium, a scheme of analysis that is a distinct advance over previous conceptions.

[MS. received March 14, 1942]

THE PSYCHOLOGY OF JOHN DEWEY

BY PAUL CRISSMAN

University of Wyoming

That Professor Dewey was among the first to construe psychological behavior in 'functional' terms is commonplace knowledge. That his 'instrumental,' and, more recently, 'operational,' account of thought or inquiry has profoundly modified educational theory and practice as well as logic is, at least in a general way, widely known. Yet it seems safe to say that comparatively few, even among psychologists, enjoy extensive first-hand acquaintance with the writings of Dewey. Indeed, this is not surprising, for he has contributed little or nothing to experimental methods in psychology, and, to psychologists whose training consists largely in the mastery of techniques, Dewey's psychology seems almost exclusively of the 'armchair' variety. Moreover, his psychological doctrines are scattered throughout his writings, and are nowhere assembled within a single pair of covers. Accordingly, a brief statement and critique of the concepts essential to Dewey's psychology should not prove wholly uninformative.

1. *Habit.*—A desirable starting-point would seem to be habit, for in Dewey habit is assigned an original and fundamental role. His contention is that habits not only constitute the content of perception, thought, meaning, object, imagination, mind, and personality or self. Moreover, it is in terms either of frustrations of, or of conflicts between, habits that he accounts for the genesis of impulse, desire, purpose, motive and consciousness.

The essence of habit, writes Dewey, consists in "an acquired predisposition to 'ways' or modes of response, not to particular acts except as under special conditions, these express a way of behaving. Habit means special sensitiveness or accessibility to certain classes of stimuli, standing predilections and aversions, rather than a bare recurrence of specific

acts" (6, p. 42). Habit denotes ". . . that kind of human activity which is influenced by prior activity and is in that sense acquired; which contains within itself a certain . . . systematization of minor elements of action; which is projective, dynamic in quality, ready for overt manifestation; and which is operative in some subdued . . . form even when not obviously dominating activity" (6, pp. 40–41).[1] In other words, habits are (1) *dynamic* and *persistent*, they are operative in some incipient manner even when not overt, they exhibit, in Duffy's phrase (14), 'a certain energy level'; (2) they are *selective*, they are responses, whether incipient or overt, to certain stimuli rather than to others; (3) they are conditioned by prior experience and are in that sense *learned* or *acquired;* (4) they are *purposive*, they tend to restore equilibrium between forces internal to the organism and environmental forces, they tend, in Muenzinger's phrase (17), 'to reconstruct the psychological situation'; and (5) they embrace not only stereotyped and repeated acts but seemingly all other complex responses as well.

2. *Character.*—Dewey describes 'character' as an 'interpenetration of habits' (6, p. 38), "a complex of unstable, opposing attitudes, habits and impulses which gradually come to terms with one another and assume a certain consistency of configuration" (6, p. 38).

Simple and commonplace though it seems, this description of the self is not free from confusion. For, on the one hand, the self is said to embrace attitudes and impulses as well as habits. Yet Dewey nowhere distinguishes attitude from habit; and, as for impulse, this term, as we shall see, denotes something so vague and slippery in meaning as to be next to useless for purposes of scientific description. Elsewhere (6, p. 138), however, Dewey represents the self as an 'integration' of certain complex and unstable emergents in nature which possess permanence or structure only as tendencies to behave in specific ways, which is but to say that the self, in so far as describable, is a complex 'Gestalt' of habits.

[1] The terms 'disposition' and 'will' are but synonyms of habit (*cf.* 6, pp. 41–42).

If so, and this would seem to be Dewey's position, it should, as far as it goes, evoke no dissent.

3. *Impulse.*—If we turn to Dewey's description of 'impulse' or 'instinct' and its role in behavior, we find him far from clear. The traits ascribed to impulse are as follows:[2] (1) Impulses are vague, primitive, groping, plastic, in themselves meaningless; they gain definite form only when incorporated into habits; (2) they are dynamic, explosive, propulsive, the original driving force of all conduct; (3) they are the pivotal points for the reconstruction of habits; they are the promise of growth, not an obstacle to it; (4) they are infinitely numerous and varied, "there is an impulse for every habit and more besides"; (5) they are 'secondary' and 'intermediary' and hence provide no explanation of conduct; (6) they are a necessary condition of the origin of thought or reflection. "Quadrupeds run, worms crawl, fish swim, birds fly." Nothing is gained by "inserting instincts to run, creep, swim and fly between the structure and the act" (10, p. 11). "The alleged instincts . . . appealed to as causal forces themselves represent psychological tendencies which have previously been shaped into habits of action and expectation by means of the very activities which they are supposed to explain" (10, p. 10).

In this account of impulse, at least one aim *seems* clear: Dewey would substitute for the mystical and causal conception of impulse a description of it as merely the dynamic and persistent phase of habit. Yet unless habit denotes some synaptic, neurological or other physiological structure or activity, this is but to say that habits are habits, that behavior is behavior. What is gained by separating off from habit one of its essential traits, upon which a new and distinct autonomy is then conferred? Such a *tour de force* seems wholly artificial. If the term 'instinct' must be retained, would it not be more accurate to describe and measure it as persistence toward some goal or as the consummatory phase of an act or system of acts?[3] Or, it might be described as

[2] See (6, pp. 107–111, 131–170) and (10, Chap. 1).

[3] The recent substitution of such terms as 'urge' and 'drive' for this, the now widely accepted description of instinct, marks a long stride in the direction of descrip-

those pervasive and persistent human and animal habits that satisfy common biological needs and desires. Despite these gestures toward a scientific description of instinct, however, Dewey does not wholly free himself from the causal conception of it; even in some of his more recent writings, especially when off his guard, instinct still lurks as a vague and shadowy force behind many of our acts. As we shall see later, this use of it is especially obvious in his account of the origin and persistence of thought or inquiry; it also pervades many of his educational writings.

Yet we believe it is precisely in his caution in regard to instincts that Dewey makes his soundest contribution to the psychology of education, namely, in his contention that we must look elsewhere than to rigid instincts for the obstacles that impede change and progress in human behavior. The most obstructive of these he finds to be *external* to the individual; they reside, for the most part, in the irrationality and conservatism inherent in current educational theory and practice, both formal and otherwise. The plastic and inchoate impulses of childhood are quickly tied to customary patterns of feeling and sentiment, value and action, so that, by the time memory develops and intelligence becomes operative, basic habits have been formed that but perpetuate the biasses and prejudices, the irrationalities and inequalities, the sentiments and values of the group in which the individual is nurtured. That recent studies of the determinants of habit and belief permit no other conclusion is now too commonplace to mention.

Even here, however, danger lurks. Although available experimental evidence permits us to speak only with the greatest caution, it is probable that physiological structures and biochemical processes, as well as responses previously learned, condition and circumscribe all present behavior, sometimes even in specific and near-measurable ways. 'Mind' is no mere 'empty slate' or 'blank tablet' upon which just any behavior may be inscribed. In any case, the de-

tive accuracy. However, to the degree in which these newer labels come freighted with the older connotations of instinct, the rechristening is purely verbal.

termination of the degree to which individual 'human nature' is plastic still awaits vast experimentation of a kind as yet banned.

4. *Emotion.*—Dewey describes emotion as "a perturbation from clash or failure of habit" (6, p. 76). Emotion, however, arises when an animal confronts a situation in which it knows not how to act or for which no adjustive response has as yet been acquired. Grant that many emotions do arise in this manner, yet they may also be induced by other means. Furthermore, we are not told about the nature of this perturbation, whether it is conscious, *i.e.*, a datum of introspection and reportable verbally, or whether bio-chemical, neurological, physiological, or some combination of these. Unfortunately it is precisely at this point that the scientific problems of emotion begin, not end.

5. *Motive.* Dewey defines a motive as "simply an impulse viewed as a constituent of habit" (6, p. 122). In the descriptive sense of impulse and habit apparently here intended, to say that men and animals are 'moved' by motives is scientifically useless and also tautological. "Hunger," *e.g.*, "is an active process not a motive to an act" (6, p. 119); hunger is no causal force that moves us to search for food, it is but a name for the search for food. "A motive does not exist prior to an act and produce it" (6, p. 120). Yet despite these statements, a motive denotes "an element in an act viewed as a tendency to produce such and such consequences" (6, p. 120). Stranger still, 'the whole concept of motives' is then dismissed as 'extra-psychological.' For it is now declared that motive "is an outcome of the attempt of men to influence human action, first that of others, then of a man to influence his own behavior" (6, p. 119). "It is an act *plus* a judgment upon some element of it, the judgment being made in the light of the consequences of the act. Instead of saying that a man requires a motive to induce him to act, we should say that when a man is going to act he needs to know what he is going to do . . . what the quality of his act is in terms of the consequences to follow" (6, p. 121).

Now the rejection of motive as a mystical 'force' in or

behind our acts is, however commonplace, commendable enough. Yet all events have some cause; indeed, to discover the 'causes' of human and animal behavior is a genuine task of psychology. To restrict motive to some conscious element within the total act which tends 'to produce such and such consequences' is open to grave objection; it restricts motive to something too narrowly 'intellectualistic' and 'moralistic' to cover all that in current psychological usage is embraced by motive.

Moreover, if motive be 'simply an impulse viewed as a constituent of habit,' then, unless impulse denote also something additional to this, motive and impulse designate the selfsame referent. To add that motive is the 'outcome of the attempt of men to influence human action' succeeds only in confusing the issue. For, if motive be an element already present and operative in some habit of an agent, then motive and impulse are one; if, *per contra*, motive be some impulse conferred upon habit, either by the act of another or by a reflective act on the part of the agent himself, then motive does operate independently of habit.

More puzzling still, why dismiss motive as extra-psychological? Grant that motive *is* difficult to describe; limit it, as Dewey tends to do, to control of means by ends; admit that it may eventually be supplanted by another descriptively more exact and experimentally more fruitful term; yet we are not thereby warranted in banishing motive to the limbo of the non-psychological. In any case, Dewey's use of motive is highly psychological.

6. *Desire.*—Scarcely distinguishable from the concept of motive is that of desire. "Desire," writes Dewey, "is the forward urge of living creatures. When the push and pull of life meet no obstacle, there is nothing we call desire. There is just life activity. But obstacles present themselves, and activity is dispersed and divided. Desire is the outcome; it is an activity surging forward to break through what dams it up. The 'object' which then presents itself in thought as the goal of desire is the object of the environment *which, if it were present,* would secure reunification of activity and the

restoration of its ongoing unity" (6, pp. 249–250). Desire is thus a phenomenon of frustration, the result of some disequilibrium between the vital activities and habits of the organism on the one hand, and external forces and things present to it as environment on the other.

As respects its genesis, this account of desire need not give us pause. But how is desire as described above to be distinguished from motive? Only, it would seem, by a redefinition of desire. Indeed, the strongly 'mentalistic' quality ascribed to desire is highly questionable. Suppose I possess a desire to go fishing. I may indulge in an imaginary excursion into an equally imaginary environment of lakes and streams, rods and fish, at present inaccessible to me; yet, such imaginary acts and objects cannot, at least for scientific purposes, constitute the essence of a generic definition of desire. For, unless desire be distinguished as a species of 'urge' or 'drive' and restricted to conscious behavior, 'conscious' content must be omitted in description of it.[4] A scientific psychology finds it adequate to describe desire as the end, terminal or consummatory phase of an act or series of acts which mark satiety. In any case, Dewey's account of 'desire' leaves much to be desired.

7. *Thought.*—Habit, impulse and desire supply us with the processes essential to Dewey's account of the origin, nature and function of 'thought' or 'inquiry,' to which we now turn.

Dewey describes thought (2, 6, 7, 8, 9, 11) as that mode of organic behavior in which prior successful experiences, conserved in present habits, control the present course of behavior in order to fulfill some desire or purpose. 'This time spread, including' also 'an anticipatory or predictive phase,' is essential to thought. Thought arises as a response to those situations in which the objects and events present therein are too numerous and conflicting immediately to evoke a satisfying response, or else in those situations in which the objects requisite for the fulfillment of some desire or purpose are to some degree lacking.

[4] Since in Dewey desire and purpose seem psychologically indistinguishable, this account of desire is equivalent to an account of 'purposive' behavior in animals and men.

Moreover, frustrations of, or conflicts among, habits and impulses arrest action and focus attention upon the 'objective components of the habits.' These objects are then 'scrutinized one by one and their potentialities noted,' with the view to discovering some act or operation that promises fulfillment of the desire or purpose. Otherwise put, thought is that activity in which an imaginative search is made for an 'adequate' stimulus or 'ideal' object which, when hit upon, will effect such a reorganization of habits as to constitute a new and integrated act as yet untried. This process is said to 'consitute the analytical phase of thought' [5] (*cf.* **6**, p. 190).

Moreover, Dewey finds the content of thought, not in 'sensations,' 'perceptions' or 'images,' but in habits. Sensory qualities, instead of constituting the content of thought, are but stimuli which evoke thought. At most, a *sensum* is but 'a sign of the direction inquiry must take;' otherwise it represents merely 'the immediate or qualitative aspect of experience' (**7**, **8**). "We think with our habits," writes Dewey. "Concrete habits do all the perceiving, recognizing, imagining, judging, conceiving and reasoning that is done" (**6**, p. 177). However, this is not to deny that we think with our concepts, for all concepts are habits. A habit functions 'conceptually' when it assumes a certain generalized function for thought (**6**, **8**), *i.e.*, when a given habit functions as one and the same response to a plurality of events or situations.

Since habits constitute the content of thought, they define both its limitations and its possibilities. Dewey states, "The more numerous our habits, the wider is the field of possible observation and foretelling. The more flexible they are, the more refined is perception in its discrimination and the more delicate the presentation evoked in imagination" (**6**, pp. 175–176). The more numerous and varied are the habits we bring to bear upon a given situation, the more searching and effective is our thinking in that situation. Suppose, to take an illustration dear to the instrumentalist,

[5] Elsewhere (**5**) Dewey presents a more systematic analysis and description of the stages of thought. Since this is widely known, we have not deemed it necessary to reproduce it here.

we consider a man lost in a forest. How may he regain his starting-point? If he *thinks*, he must canvass the potentialities of those of his prior-formed habits that are evoked by the actual situation and 'weigh' the relative promise of each toward effecting his return. He shouts aloud, but no voice answers; he looks for familiar landmarks, but sees none; he looks at his map, but cannot locate his position thereon; he 'thinks of' his compass, but finds he has left it behind. Finally, he notes the bearing of the sun, looks at his watch and observes the time of day and, by a simple inference, regains his sense of direction. Acting upon this latter clue, he succeeds in returning to his starting-point. Thought has then discharged its function and disappears.

With this account of thought as thus far described we are not disposed to cavil. But now confusion enters. For, declares Dewey, ". . . habit does not, of itself, know. It just lets go. Habits by themselves are too organized, too determinate to need to indulge in inquiry and imagination" (6, p. 177). "A certain delicate combination of habit *and impulse* [6] is requisite for observation, memory and judgment" (6, p. 177). The respective roles assigned to habit and impulse in reflective acts are as follows. Impulse "determines the direction of movement. It furnished the focus about which reorganization swirls." It "defines the peering, the search, the inquiry. During this search old habits supply the content, filling, definite . . . subject matter" (6, p. 179). Impulse "begins as vague presentiment of what we are going towards. As organized habits are deployed and focussed, the confused situation takes on form, it is 'cleared up' . . . the essential function of intelligence. Processes become objects. Without habit there is only irritation and confused hesitation. With habit alone there is machine-like repetition. . . . With conflict of habit and release of impulse there is conscious search" (6, p. 180).

Yet is not this seeming ascription of a separate and auton-

[6] *Italics mine.* This reciprocal interplay between impulse and habit as the necessary condition of thought is found elsewhere in Dewey; see especially (3, p. 404), and (6, pp. 254–255, 195–198).

omous role to impulse to eat of the forbidden fruit? If, as we have seen, impulse is but the dynamic phase of habit, what is meant by saying that a delicate combination of habit *and* impulse is required to evoke thought? Since habits themselves are dynamic, may they not alone perform this function? Moreover, if impulse is released by conflict of habits,[7] how can impulse be a property of habit? This Jekyll-and-Hyde role performed by impulse would seem to stem from the effort to free impulse from mystical and causal connotations on the one hand, and the retention of these very meanings in the use of it on the other.

How, too, can impulse determine the direction of movement? For direction is a property of habit; to be a habit is to have direction. If, on the other hand, impulse designates that autonomous element in habit which gives direction to the whole, then the definition runs afoul of a type of atomistic psychology which Dewey himself was among the first to repudiate. Worse still, impulse is scarcely the word with which to denote the 'directional' phase of behavior. The dilemma is now obvious: As an autonomous and causal force, impulse designates no empirical referent; as a name for the dynamic phase of habit, the concept is superfluous; in either sense, the term should be dropped. In sum, the suspicion arises that far too many psychological rabbits are made to emerge from the magic hat of habit.

As respects the instrumental function of thought, Dewey has various formulas (2, 6, 7, 8, 9, 11). In its original or biological sense, thought is adaptive in a purely organic manner, just as are hand and eye. It differs from the responses of the latter in that the latter respond only to stimuli with which the organism is in actual contact, whereas in thought the organism reacts to meaning,[8] to objects in their

[7] The result of this conflict, as we have seen, is emotion. But if so, why not now call it this? Because, it would seem, Dewey has bowed to an Idol of the Theatre; impulse or instinct, not emotion, is the term traditionally employed to designate the 'force' or 'forces' that move us to act.

[8] In this sense thought is described as 'the tool of tools' (4, p. 186). Unfortunately the metaphor is both inept and ambiguous; inept, because a machine never permits the variation and experimentation characteristic of thought; ambiguous, because this might mean (1) that thought is the most valuable of tools, or (2) that thought is the one tool

absence, thereby enormously extending the range of its adaptations. In terms of habit-formation, thought denotes those processes, evoked by conflict between or among habits and impulses, in which discord and frustration terminate in the development of new and successful operations or habits, as described in detail above. In terms of values, the function of thought consists in rendering values, enjoyments or consummations more numerous, more enhanced and more 'secure in experienceable existence' (*cf.* 4, Chap. 10). Translated into moral terms, "the ultimate ground of reflection is to enable men better to make choices of things as good or bad" (4, p. 33).

8. *Mind.*—Turning to Dewey's account of 'mind,' we note that the meaning of mind is first contrasted with that of matter. "That to which both mind and matter belong," we are told, "is the complex of events that constitute nature." These terms denote functional characters: "Nothing but unfamiliarity stands in the way of thinking of both mind and matter as different characters of natural events, in which matter expresses their *sequential* order, and mind the order of their meanings in their logical connections and dependencies" (4, pp. 74–75). Matter is but a name for the quantitative and measurable character of events; mind denotes their qualitative and indeterminate phase. Without the recurrent and the uniform, there is nothing we can properly call matter; without the incomplete and the uncertain, nothing we can call mind. Otherwise stated, "The distinction between the physical, psycho-physical, and mental is one of levels of increasing complexity and intimacy of interaction among natural events" (4, p. 261). The doctrine that matter, life and mind represent separate kinds of Being springs from a false substantization of function (*cf.* 4, p. 261). Mind is no thing which behaves, mind *is* behavior; there are no organisms with minds, there are but organisms that at times behave 'mentally.'

by which all other tools are made. Assume the latter to be the meaning intended, even then, to continue the metaphor, would it not be more accurate to describe thought as *a* or *the* whole mental machine-shop?

Unhappily the distinction between mind and matter thus far drawn is more significant for what it denies than for what it affirms. There is no need to belabor mystical and substantive conceptions of mind, for these already have gone the way of all flesh. Yet how can mind ignore the 'sequential' character of events? For all events, mental or otherwise, are sequential; indeed to discover sequential relations between or among mental processes is part of the business of psychology. Moreover, to describe mind as qualitative and matter as quantitative is equally misleading. For by qualitative Dewey means the unique, the individual, the irrational in events (*cf.* 4). Yet to be an event, *whether* mental or physical, is to be unique, individual, to some degree irrational. Furthermore, it is a major contention of Dewey that the unique cannot be known; it may be suffered, enjoyed, undergone, but not known. Consequently, if *all* events, both mental and physical, are to some degree unique, as in fact they seem to be, then mind is ubiquitous; if, moreover, mind is equivalent to the unique in events and the unique is declared unknowable, then *exeunt* all hope of knowledge of the mental. A similar fate threatens mind when described as the incomplete and the uncertain. For knowledge, psychological as well as otherwise, consists in the discovery of uniform or invariant relations between or among things and events; if we can know only these, and these constitute the essence of matter, then we can know only matter.

Yet this seeming *impasse* need not halt us; clearer light is shed upon Dewey's conception of mind if we turn to his account of its genesis. "The causal antecedents of its existence," he holds, "is a substratum of psycho-physical actions," for, he adds, "in contrast with lower organisms the more complex forms have distance receptors and a structure in which activators and effectors are allied to distance much more extensively than to contact receptors. What is done in response to things nearby is so tied to what is done in response to what is far away that a higher organism acts with reference to a spread-out environment as a single situation" (4, p. 279). Moreover, "in all these higher organisms what

is done is conditioned by consequences of prior activities; we find learning or habit-formation. Thus an environment both extensive and enduring is . . . implicated in present behavior. Operatively speaking, the remote and the past are 'in' present behavior making it what it is" (4, p. 279). This presence of the past in present behavior, it need scarcely be added, is the mark of mind.

Unhappily, mind is now invested with a narrower meaning. This is accomplished by restricting it to a function of speech or language habits. "Since animals are connected with one another in inclusive schemes of behavior by means of signalling acts, a joint action made possible by signalling occurs" (4, p. 280). In the human animal, "this process becomes language, communication, . . . in virtue of which the consequences of the experiences of one individual are integrated in the behavior of others. With the development of recorded speech, the possibilities of this integration are indefinitely widened. When consequences that are unexperienced and future to one agent are experienced and past to another with which it is in communication, organic prudence becomes conscious expectation, and future affairs living present realities. Learning or habit-formation presents thereby an integration of organic-environmental connections so vastly superior to those of other animals without language that its experience appears to be superorganic" (4, p. 280).

Yet with the formation of habits or interests,[9] new needs and wants develop, new points of contact with the world are established. "Each habit demands appropriate conditions for its exercise and when habits are numerous and complex, as with the human organism, to find these conditions involves search and experimentation; the organism is compelled to make variations, and is exposed to error and disappointment" (4, p. 281). Hence novelty, instability, frustration, emergence of unexpected combinations occur. "The more an organism learns," *i.e.*, the greater the number of habits or interests it acquires, "the more it has to learn to keep itself going" (4, p. 281). The more extensive the learn-

[9] 'Interest,' for Dewey, is equivalent to habit.

ing, or the more complex and the more highly ramified the process of habit-formation, the more extensive and the more diversified is the domain of behavior called 'mind.'

Mind is thus no mere private and subjective phenomenon. The tendency to construe it as such, and to consider it as disparate in nature and locus from other events, and composed of some mystical 'mental stuff,' is traceable, writes Dewey, 'to the substantization of personal soliloquy.' "Soliloquy," however, "is the product . . . of converse with others; if we had not talked with others and they with us, we should never talk to and with ourselves. Because of converse, various organic attitudes become an assemblage of persons engaged in converse: . . . through speech a person dramatically identified himself with potential acts and deeds; he plays many roles. Thus mind emerges" (4, p. 170).

How, it remains to be asked, is mind known? By some act of intuition, direct acquaintance, or introspection? If by these means be meant bare awareness, immediate self-evidence, a kind of 'having'; if by these be meant that 'to be' is 'to be known,' then we possess knowledge of mind by none of these means. "Indirect approach through inquiry, with formation of hypotheses and by experimental observation" is the sole road to the knowledge of the mental as of all other things (*cf.* 12, 30). Indeed, to know what red color is, "an extensive field of knowledge, factual and theoretical, is a necessary condition" (12, 33). To discover the nature of the mental, we need "the best conclusions . . . reached about behavior by the complex of conditions constituting culture, including communication or language" (12, 34). And, Dewey might have added, we need much knowledge of neurology, physiology, bio-chemistry and physics as well. Mind is no datum of sensory awareness; it is a system of 'conscious constructs.'

With most of these criteria of mind and of the mode of our knowledge of them the majority of psychologists would doubtless agree. Yet an obvious ambiguity pervades this account of mind. On the one hand, mind is declared to embrace all organic behavior which is novel, contingent and unpredictable.

A similar denotation is possibly implied when mind is described as a function of learning or habit-formation. On the other hand, mental behavior is restricted to those organisms that exhibit speech or language communication; indeed, language habits are declared to be "the cherishing mother of all significance, the precondition of the existence of meaning and of mind" (4, pp. 77–80). The logic of this latter view runs as follows: Mind denotes behavior-with-meaning, but meanings are functions of language; since human organisms alone react to language symbols, mind is restricted to human behavior.[10] The wider of these views is the familiar pragmatic or instrumental description of mind, the latter and more restricted view of it is not as yet widely held.[11] Indeed, this socio-linguistic theory of the origin and role of mind is highly dubious. For, unless language habits be extended to embrace all responses to gestures and other physical signs, may not at least some meanings arise and function independently of language? A dog learns to cower in response to a certain gesture; I pick up the raincoat in response to the perception of a dark cloud. True, these reactions become articulate and their meaning communicable only when framed in terms of language symbols, yet it is one thing to say that meanings may be communicated only through language symbols, and quite another thing to say that meanings arise only in, and are learned only in virtue of, language communication. In other words, it is one thing to react meaningfully, and another thing to describe and communicate the meaning in terms of speech. Indeed, we believe it would not be difficult to show that many meanings are learned prior to and independently of communication.

9. *Consciousness.*—Passing to Dewey's description of 'consciousness,' we discover also two conflicting meanings assigned to it. First, consciousness designates "certain qualities in their immediate apparency . . . such as are,

[10] This view of mind is borrowed apparently from G. H. Mead. Incidentally, Mead (16) presents what we believe to be the most detailed and persuasive statement of this socio-linguistic theory of the origin and function of mind.

[11] As to the source of this ambiguity in Dewey's account of mind, we find it convenient to postpone the discussion to a later section.

. . . usually termed feelings" (4, p. 298). Here consciousness denotes absence of meaning; it exists "apart from the . . . employment of signs, or independently of communication" (4, p. 298). As bare immediacy, it is inner, private, indefinable, "not because there is some mystery in or behind it, but for the same reason that we cannot tell just what sweet or red is: it is something had, not communicated and known" (4, p. 307).

In the second and narrower sense, consciousness denotes "meanings actually perceived, awareness of *objects;* being wide-awake, alert, attentive to the significance of events, present, past, future" (4, p. 298). In this sense, consciousness is awareness of meaning, 'the having of ideas.' But meanings or ideas are declared to be functions of language, and language involves two or more persons in a 'shared undertaking.' As such, consciousness is a function of what Ellwood calls 'social inter-stimulation and response' (15). In terms of mind or meanings, "consciousness, an idea, is that phase of a system of meanings which at a given time is undergoing transformation" and redirection. "It is the *meaning* of events in the course of remarking"; it is the "double relationship of continuation, promotion . . . , and of arrest, deviation, need of supplementation, which defines the focalization of meanings which is consciousness, awareness, perception" (4, p. 306). "The *immediately* precarious, the point of greatest immediate need, defines the apex of consciousness its intense or focal mode." Consciousness implies both continuity and discontinuity; "if perceptions were not a series of terms with respect to one and the same unfolding meaning, we should not attend to them or be aware of them. If, on the other hand, there were no variations to compel suspense, . . . there is no perception, no consciousness" (4, p. 308).

This double meaning of consciousness may now be set in sharper relief. On the one hand, it is described as awareness, attention, awareness of events without involvement of meaning. On the other hand, it is restricted to perception, perception of meaning, awareness of objects, the having of ideas, the meaning of events in the course of remaking. In

its broader sense, consciousness is awareness of events, in its narrower sense, awareness of objects or meanings.[12] What, then, are 'objects' and 'events,' and how are these to be distinguished?

To this question Dewey replies as follows. We are conscious of objects, not of events as such (*cf.* 4, p. 318). For objects are events-with-meaning, configurations of meanings, the cognitive aspect of events. Events are indefinable, indefinable because meaningless. For events are 'immediate qualities of sentience,' the temporal flux of sensations and feelings. Events denote the bare stimuli which evoke responses; while they evoke the responses which constitute meanings, they are not in themselves meanings.[13]

Consciousness is thus a property of behavior, and behavior is response to objects, to environment-with-meaning. This, coupled with the fact that consciousness is dependent upon organic structures and activities which, in turn, are dependent upon extra-organic things and forces, negates all attempts to separate consciousness from physiological, bio-chemical and physical structures and activities. This does not mean that consciousness is deducible from, or reducible to, vital and physical processes. The 'impassable gulf' is there, asserts Dewey; it exists, however, not between two entities possessing uniquely different properties, but only as a special case of the general impossibility of reducing the qualitative to the quantitative (*cf.* 4, p. 349).

Thus there is an obvious difference between mind and consciousness, meaning and idea. "Mind denotes the whole system of meanings as they are embodied in organic behavior"; consciousness denotes 'awareness or perception of meanings,' the perception of events in their meaning, 'the actual having of ideas' (*cf.* 4, p. 303). Accordingly "the

[12] This dual meaning of consciousness is no mere oversight, it is intentional. Indeed, Dewey warns us (4, p. 298) that it is futile to attempt to reduce either to the other. Happily there is no need to do so; the confusion resides in Dewey's use of terms, not in the referents denoted by them. Would it not be better here to employ two terms, say consciousness and self-consciousness, or, if this pair be obnoxious, awareness and consciousness?

[13] Meanings, as we shall see, are operations, responses to events, the uses to which things are put.

greater part of mind is only implicit in any conscious act or state; the field of mind, of operative meanings, is enormously wider than that of consciousness. Mind is contextual and persistent; consciousness is focal and transitive; mind is structural, substantial, a constant background and foreground; consciousness is intermittent, a series of flashes of varying intensities" (4, p. 303). To illustate, as the intermittent flow of a geyser is symptomatic of a continuous activity underneath, so consciousness, as a system of 'flights' and 'perchings,' represents the specific area of rise and fall wherein an attentive act, within the relatively stable and continuous body of meanings, reached its nodal point. To describe the properties of consciousness, such words as apparency, vividness and intensity are needed; to describe those of mind, the terms order, organization and coherence seem most accurately descriptive (*cf.* 4, pp. 304–305).

10. *Meaning.*—If mind is the system of meanings, and if consciousness [14] is awareness of specific meanings in their movement, *what then are meanings?* "A meaning," writes Dewey, "is but a method of action, a way of using things to a shared consummation" (4, p. 187). Meanings are "rules for using and interpreting things; interpretation being always an imputation of potentiality for some consequence" (4, p. 188). The "meaning of an object is the change it requires in our attitude, the meaning of an idea is the change it *as* attitude effects in our objects" (13, p. 89). All events have effects, and, "since meaning is awareness of these consequences before they actually occur," to reflect or think is "to take events which brutally occur and brutely affect us, and to convert them into objects (meanings) by means of inference as to their probable consequences" (*cf.* 4, pp. 324–325). Since meanings are but names for "what events portend," the anticipated consequences of our responses to events, the function of reflection is to invest events with meaning, with the view to the deployment of it toward the consummation of some desire or purpose.

[14] While consciousness, as we have seen, possesses also a wider meaning, it is in this sense that Dewey most often employs the term.

11. *Conclusions.*—We conclude with a few comments upon the concepts and relations between them as described above. With Dewey's behavioral and instrumental account of thought, mind and consciousness, as well as of many of our meanings, we are not disposed to quarrel. Whether the subject-matter of psychology should be restricted to meaningful behavior, as Dewey seems often to imply, is largely a matter of definition of terms and of fruitful division of labor. If psychology is to fulfill its aspiration to become a natural science, it must do more than provide neat labels for behavioral acts; it must also seek to discover causal relations or correlations between these acts on the one hand, and their social, physiological, bio-chemical and physical correlates on the other. The recent trend toward the quantification of psychological knowledge is but the recognition of this fact. Furthermore, since psychological phenomena are the most complex in all nature with the possible exception of the social phenomena, some measure of confusion and uncertainty as respects its specific subject-matter is to be expected.[15]

As we have seen, a basic ambiguity pervades Dewey's description of meaning, mind and consciousness. This ambiguity, to repeat, is intentional, for it is the necessary reflex of a more fundamental ambiguity, one implicit in all instrumental and operational accounts of thought and of meaning. These latter all speak of significance or meaning as consisting wholly of organic responses. This view derives its plausibility from preoccupation with genetic method. That *many* of the meanings assigned to objects consist of responses made to them cannot be denied; a pen is, among other things, something to write with. But if the *entire* meaning of an object be restricted to those operational meanings developed in the course of our responses to it, the view becomes highly implausible.[16] For, since the entire meaning of an object would then consist solely of the responses made by some organism to it, and since the object *consists solely of meanings*, no object

[15] In a forthcoming article, we shall attempt to specify the relation of psychological subject-matter to that of the other natural sciences.

[16] Elsewhere (1) we present a more detailed analysis and criticism of the operational theory of meaning.

could exist prior to the responses, which is equivalent to saying that we create the objects to which we respond. Yet a world of things and events must exist external to the organism and antecedent to it, otherwise organic behavior deploys *in vacuuo* and psychological phenomena hang by their own boot-straps. When Dewey admits that "in itself the object is just what it is experienced as being, hard, heavy, sweet, sonorous, agreeable, tedious," as he does not only here but in many other passages as well, he succeeds only in cutting the Gordian knot, he does not untie it. In sum, the ambiguity which pervades Dewey's use of meaning, mind and consciousness seems to stem from the attempt to graft a logic whose terminus is *bio-* or *psycho*-centrism upon a common-sense realism for which a world of determinate things and events is assumed to hover in the background all the while.

We turn to Dewey's use of concepts generally. As we have seen, the properties and roles assigned to impulse, emotion, motive and desire are far from clear and unequivocal. In a number of cases the meanings of these tend even to overlap. This is noticeable also, though in lesser degree, in Dewey's use of the terms perception, meaning, object, mind, consciousness and self. Nor is this any cause for surprise, for vagueness and confusion would seem to be the inevitable outcome of all attempts to retain the concepts of popular psychology and to employ them in their common-sense meanings as adequate for scientific purposes. Since Dewey's system represents an unusually thorough-going effort to construct a systematic psychology on the level of common sense, it is most instructive in at least one respect: It reminds us once more of the necessity for a most searching criticism of concepts in current use ere psychology can assume a place as an adult member of the community of natural sciences. Psychologists are many and increasingly critical; experimental techniques are legion and highly refined; much valuable ground-work has already been done in criticism of concepts. Innocence cannot longer be justified on the ground of childhood or youth. In truth, the present sharp increase in the number of critical studies devoted to this problem is evidence

that a new renaissance is fast approaching if indeed it is not already upon us.

Finally, we would reject the concept of habit as fundamental to psychology. As we have seen, impulse, emotion, motive, desire, perception, imagination, thought, meaning, object, mind, consciousness and self are described as processes integral to, or else as functions of, habits. Yet if psychology is to qualify as an empirical science its concepts should fulfill the following functions: (1) they must be clear and univocal in meaning; (2) they must describe or explain the phenomena referred to by them; (3) they must be consistent [17] with one another as well as with our knowledge in other sciences; and, (4) they should be fruitful, *i.e.*, lead to the discovery of new hypotheses and new facts. In virtue of the loose, shifting and highly complex character assigned to habit, we find Dewey's use of it deficient in these respects to such a degree as to render the term largely useless for the science of psychology. The main difficulty lies in the very complexity of habit; if all human and animal behavior be construed as some phase of function of habit, as it is at least verbally possible to do, confusion is the outcome. The error here would seem to consist in taking as fundamental and constitutive what is at once the most complex and derivative of all psychological concepts, an error which, ironically enough, Dewey never tires of warning us against. Basic concepts should be as simple in meaning and as few in number as is possible to achieve, not complex and consequently overlapping as is all too often the case in Dewey's use of them. Indeed, the suspicion is now confirmed that far too many psychological garments are made to hang upon the peg of habit.

REFERENCES

1. Crissman, P. The operational definition of concepts. Psychol. Rev., 1939, 46, 309–317.
2. Dewey, J. *Creative intelligence* (Symposium). New York: Henry Holt, 1917. Pp. 3–70.

[17] By 'consistency' is meant logical consistency, *i.e.*, two or more concepts, propositions or hypotheses are consistent with one another when no one of them implies the denial of any other of them.

3. ——. *Democracy and education.* New York: Macmillan, 1935. Pp. xii+434.
4. ——. *Experience and nature.* Chicago: Open Court, 1925. Pp. xi+443.
5. ——. *How we think.* Boston: D. C. Heath, 1910. Pp. vi+224.
6. ——. *Human nature and conduct.* New York: Henry Holt, 1922. Pp. vii+336.
7. ——. *Influence of Darwin on philosophy and other essays.* New York: Henry Holt, 1910. Pp. vi+309.
8. ——. *Logic.* New York: Henry Holt, 1938. Pp. viii+546.
9. ——. *Reconstruction in philosophy.* New York: Henry Holt, 1920. Pp. vii+224.
10. ——. *The public and its problems.* New York: Henry Holt, 1927. Pp. vi+224.
11. ——. *The quest for certainty.* New York: Minton, Balch, 1929. Pp. 318.
12. ——. How is mind to be known? *J. Phil.*, 1942, **39**, 29–35.
13. ——. What does pragmatism mean by practical? *J. Phil.*, 1908, **5**, 89.
14. Duffy, E. The conceptual categories of psychology: a suggestion for revision. Psychol. Rev., 1941, **48**, 177–204.
15. Ellwood, C. A. *The psychology of human society.* New York: D. Appleton, 1931. Pp. xvi+519.
16. Mead, G. H. *Mind, self and society.* Chicago: Univ. of Chicago Press, 1936. Pp. xxxix+519.
17. Muenzinger, K. F. *Psychology.* Denver: The World Press, Inc., 1940. Pp. xiii+343.

[MS. received February 17, 1942]

LEVEL OF ASPIRATION AS A METHOD OF STUDYING PERSONALITY [1]

I. A CRITICAL REVIEW OF METHODOLOGY

BY JULIAN B. ROTTER

Norwich State Hospital

I. Introduction

Within recent years that aspect of psychological investigation denoted by the term 'personality' (in the restricted sense of stable patterns of behavior of the individual as a whole other than those concerning intellectual and motor abilities) has been growing rapidly. Psychologists who call themselves clinical, abnormal, social, child, or developmental are concerning themselves more and more with an experimental or observational, rather than a rationalistic, approach to these problems. However, due to a dearth of experimental methods in the clinical and experimental fields, results are meagre and often contradictory. Inventories, highly developed on the side of reliability but often weak in validity, have limited use in experimental studies and even more limited use in the clinical field. However, some new methods are appearing (5) such as matching techniques based on interviews and clinical observations, projective types of tests such as the Thematic Apperception, Rorschach, Drawing, Free Design, Comic Strip, and Tautophone. There are also appearing certain specialized techniques originated by students of Lewin such as resumption and level of aspiration. All of these methods require considerable further study and analysis before they can become useful as experimental or clinical tools.

[1] This review forms part of a dissertation submitted to the faculty of the Graduate School of Indiana University in partial fulfillment of the requirements for the degree, Doctor of Philosophy. I am indebted to Dr. C. M. Louttit, who directed the dissertation, and to Mr. David Shakow, Chief Psychologist at the Worcester State Hospital, who gave much aid and encouragement in the initial stages of this investigation.

The level of aspiration technique in particular has aroused considerable interest and experimentation. This technique, concerned as it is with the effects of success or gratification, and failure or frustration in a dynamic situation has potential importance in both the theoretical study of personality development and in the field of clinical diagnosis and description. In its broadest sense, the purpose of this series of studies is to analyze as far as possible some of the factors operating in the level of aspiration situation with the aim of further evaluation of its possible usefulness in these areas. Because of its dynamic nature, this technique appears to be a useful method for the investigation of the nature of the so called general traits, reaction systems or behavior patterns.

Although differing considerably among themselves, experiments concerned with level of aspiration have in common a specific basic procedure. This procedure may be described as follows: A subject is confronted with some task and either before or after practice he is asked to make a statement of how well he will do on the task. After failure or success in reaching this explicitly stated goal he is asked to make another estimate. This may be repeated several times. Through this procedure it is possible to study, fairly objectively, the effect of success and failure on the explicitly set goals of an individual, where success and failure are defined as reaching or not reaching the previously set goal.

In speaking of level of aspiration as a technique, the writer is attempting to remain within an operational frame. Although the 'level of aspiration' has been referred to as a concept, as an attribute of individuals, and as a kind of response in various experiments, these studies differ so greatly in their assumptions, procedures and aims that there is little to bind them together, other than the fact that somewhere in their experimental procedure they use the skeleton technique stated above. In general one can group these researches into three divisions on the basis of their purposes. The first group is interested in determining principles applicable to the theory of personality. The second is interested mainly in the technique itself, its reliability, the generality of the

response from one task to another, and the validity of the assumptions usually made in interpreting the results. The third group accepts the technique as a valid measuring device of some kind and uses it to study other variables. Thus, there have been investigations of the level of aspiration behavior of feebleminded children (21), schizophrenics (36), and manic depressives (11). The technique has also been applied to studies of motivation in school children (3), vocational aspiration (33), academic factors (7), and personality traits (14).

The present paper, unlike recent reviews (17, 22), examines only those studies which have concerned themselves, in whole or part, with an evaluation of the technique or method itself. Those studies where a level of aspiration method was used solely to study other variables are not considered in the discussion but are listed in the bibliography.

II. Early Investigations

The history of studies on the effects of success and failure upon changes in the individual's stated goals can hardly begin with the first major research in which the term level of aspiration was used. Previous theoretical and experimental studies on success and failure have been related to this subject as well as clinical descriptions by Freud (30) and particularly Adler (2) and more recent works on frustration, as for example those of Rosensweig (37), Dollard and Doob (9), and Doob and Sears (10). However, the present series of investigations can be clearly traced to the work begun by students of Lewin who were interested in formulating laws concerning goal responses. Dembo (8) first mentioned the term *Anspruchsniveau*, or level of aspiration, in a study of anger where subjects were observed directly in a dynamic situation. Hoppe (29) followed this study with a similar one where direct observation of behavior was made but the concern was wholly with the laws pertaining to the change of goals following success and failure.

Hoppe, however, failed to distinguish clearly between implicit and explicit goals although recognizing that the

subject's immediate or momentary goal was different from his 'ideal' goal. The reality of the ideal goal could be changed by experiences of success and failure. He noted also that failure depressed and success elevated the immediate goal. As Hoppe was not primarily concerned with individual differences, he failed to consider the effect of different tasks or different instructions upon the changes in level of aspiration, although he noted that vast individual differences existed in what his subjects considered success and failure. Hoppe also expressed the belief that this technique had possibilities as a method of studying personality traits and listed ambition, cautiousness, self-confidence, fear of inferiority, and lack of sufficient courage to face reality as traits involved.

In a later study Hausman (26) veered from the theoretical approach of Hoppe and concerned himself with the technique 'as a test to evaluate some personality traits.' He may have recognized some of the difficulties with Hoppe's instructions although he wrote no direct criticisms. He did, however, change the instructions so that a 'bid' was made each trial, the subject being penalized if he fell below his bid and receiving no credit over it if he scored higher than his bid. Hausman also set a definite number of trials and a single task, dart throwing, was used. Like Hoppe, his approach was largely qualitative, although the raw data he obtained permitted numerical treatment. Hausman, however, was concerned primarily with the different patterns of response shown by his subjects and discussed them in relation to such traits as instability, perseverance, stubbornness, impaired judgment, and cautiousness.

The trend toward the investigation of individual differences was continued by Frank whose approach, characteristically American, was quantitative. Frank (13, 14, 15, 16) was concerned with both the theoretical value of the results of studies using this technique and with the possibilities of the technique itself as a method of studying personality. Consequently he investigated such subjects as the reliability of the measures, the generality of the individual traits that determined the response in the situation, and the possible implications of other factors. Although he failed to clarify the

difficulties encountered due to individual differences in the interpretation of the instructions and in the selection of tasks, he did begin the standardization and quantification of the results so that they would be amenable to further investigations. Frank defined level of aspiration operationally as "the level of future performance in a familiar task which an individual, knowing his level of past performance in that task, explicitly undertakes to reach" (16, 119). In doing this he was getting away from the implicit hopes, wishes and desires that Hoppe was interested in but unfortunately neither he nor following investigators who accepted his work realized that the explicit factors he was studying were not necessarily the same as the implicit ones Hoppe was interested in. Frank was concerned mainly with one measure, "the difference between the average level of aspiration and median level of performance" (16, 122), although he also suggested, but made little use of, a measure of 'rigidity' (in the language of Lewin), which could be obtained by dividing the number of times the subject shifted or changed his aspiration by the number of trials.

From his researches Frank found the test-retest reliability for the difference score for three different tasks to range from .26 to .72. These, however, were rank order correlations based on twelve subjects. Similar rank order correlations were obtained between the difference score on any two of the tasks and any of the two tests. These ranged from −.08 to + .70. He concluded from an analysis of these results that there was some generality of the traits determining the responses. Frank also showed that: (1) the aspiration estimates differed from random guessing, (2) depending on the degree of similarity between the tests, performance in one task affected the aspiration level in another, and (3) some suggestion of sex differences in behavior was present in reactions to the level of aspiration situation. Like Hoppe he agreed that the involvement of the 'ego' was determined by three needs: (1) the need to keep the aspiration level as high as possible regardless of level of performance, which tends to drive aspiration level above performance; (2) the need to make the level of aspiration approximate the level of future performance as closely as

possible (the need to keep in contact with reality) which tends to keep the level of aspiration at the level of performance; (3) the need to avoid failure, which tended to drive the level of aspiration below the level of performance. The first and third need arise from the more primary need to keep the 'I' or 'ego' level as high as possible.

Unfortunately, in these experiments, most of which were with insufficient subjects, Frank's conclusions were not fully born out by his statistical findings, but nevertheless many following investigators accepted his assumptions and conclusions and used the level of aspiration as a method of studying such problems as the nature of psychoses, academic success, and other social as well as 'personality' factors, while the technique itself was subject to many uncontrolled and unknown factors.

About the same time Jucknat (31), using a technique in which the subject chose one of a series of mazes, graded in difficulty, without any verbal committments, found that success and failure in one field may importantly displace the level of aspiration in another field, upward or downward, if there is a definite dynamic relationship between the two fields and a not too fixed level of aspiration in the second field.

III. Recent Investigations

These indications that the nature of the task and previous experience with the task might influence behavior in the situation was made clearer by studies by Chapman and Volkmann (6) and Anderson and Brandt (3). Chapman and Volkmann demonstrated that knowledge of what others did upon a test (where the subject has not yet actually performed) will markedly influence the subjects' stated aspiration depending upon how they evaluate the other people whose score they know. In this experiment one group of subjects was told that, for a test with a maximum of 50 points, experts scored at 37 points; a second group was told that subjects similar to themselves scored 37; a third that 37 was the average for WPA workers; and the fourth was given no such suggestion. The group that was told that experts scored 37 had the lowest

aspiration level (**23**); those told WPA workers scored 37 had the highest (**33**); against their own average they aspired to 31; and with no suggestion they aspired to 26. Apparently there is another influence here, as well as the social one, namely, a tendency of subjects to aspire towards the middle of the distribution when they do not actually know what they themselves can do, for not even in the case of the group aspiring against the WPA workers did the estimates come up to the suggested 37, while the estimates of the no-suggestion group were practically at the middle of the distribution.

Anderson and Brandt examined level of aspiration in a group of school children who knew not only their own past performance but could and did compare it to all of the other children in the class. Under these conditions the future estimates of all groups determined by the height of past performance tended toward mediocrity. The group with the highest ranks stated future goals significantly lower than their achievement; the group with the lowest achievement ranks stated goals significantly higher than their past achievement.

In a more recent study Gould (**22**), critical of the assumption that the explicit goals set by the subjects corresponded to the implicit goals of Hoppe, and of Frank's statement of the generality of the trait involved, set up an experimental investigation using six tasks given in two sessions and followed the regular experimental period with an interview of each subject. Although she recognized the difficulty with the instructions being used, she could not improve upon them, and rejected Hausman's instructions on *a priori* grounds. Correlating the difference score for the same subjects and different tasks, some of which dealt with familiar and some with unfamiliar abilities, she found a median intercorrelation of the difference score of only .29. The procedure was so set up, however, that success or failure in one task could easily influence the relationship between performance and estimate in another so that her six tasks might represent six trials within a larger level of aspiration situation. Indeed, the intercorrelations between tasks were almost twice as great when given in the same session as compared to those between tasks given in another session.

Gould did succeed in emphasizing that the nature of the task and situational setting influenced the behavior, but she failed to indicate whether or not there were relatively stable personality factors interacting with these situational differences. She also, particularly in her interview material, showed how the subject's interpretation of the instructions varied. Not only were explicit goals quite different from implicit goals, but the relationship between explicit and implicit goals differed widely from subject to subject. Gould, using the median of the difference scores for each individual, failed to find any significant correlation between these scores and grade level, Thorndike Intelligence Examination scores, and scores for introversion and dominance from the Maslow Social Personality Inventory.

Gardner (19), accepting an operational approach as well as an operational definition of level of aspiration, studied the explicitly stated goals for their own sake. He introduced as a control in the situation a prearranged series of performance scores which were the same for each individual. He computed a series of indices all relating to the difference between his report to the subject of performance and the subject's estimates. Contrary to Gould, he found a decided generality in response with an average intercorrelation on his main index of .57 for four tasks. More significant, perhaps, he reported that "Discrepancies between aspiration level and performance level tended to be positive but low when performance was improving rapidly and markedly higher when the performance was falling. Individuals tended to maintain the same rank within the group in respect to discrepancy scores when the performance curve was rising as well as when it was falling" (19, 621). The statistical evidence on which the latter statement was based was not presented.

Gardner (18) failed completely to obtain any relationship between personality variables as measured by rating scales with four raters and his eight indices in the level of aspiration situation. Although adding a control on the side of performance, Gardner's technique still failed to control on the side of instructions, was too rigid to permit the free expression of be-

havior upon the part of the subjects, and relied too heavily on indices which were based on the limited sample of a single estimate which were concerned only with the relationship between aspiration and performance.

While the present studies were in progress Sears (42) published a report of an investigation of the level of aspiration behavior of academically successful and unsuccessful children. Sears solved the problem of previous attitude and experience with a task by selecting her subjects on the basis of the nature of their previous experience with the tasks. She had three groups of limited samples but matched for age, sex, and intelligence: Group 1 unsuccessful in all school subjects, Group 2 successful in all, Group 3 successful in reading and unsuccessful in arithmetic. The experiences of success or failure were of long standing and Sears plausibly assumed that these groups could be called self-confident or lacking in self-confidence with reference to tests in a specific subject matter. The children received aspiration tests in reading and arithmetic. She found, as might be expected from earlier work, that the children with a history of failure in a subject had higher difference scores, on the average, than successful ones. She also found, however, that they differed even more markedly in the spread of their difference scores, the unsuccessful children making up almost all the cases at the extremes of the distribution. Experimentally induced success brought the reaction of all subjects into a more homogeneous but still differential distribution than did the neutral conditions of stimulation. It was not made clear whether the variability of the failure group was due to their specific previous experience with the subject matter of the test or whether both the tendency of the difference scores to be at the extremes of the distribution, and the academic failure in spite of equal intelligence were related to some third, common factor.

Hertzman and Festinger (27), Hilgard, Sait and Margaret (28), and Gould and Lewis (25) have all clearly substantiated the work of Anderson and Brandt and Chapman and Volkmann which showed the importance of social standards in the setting of explicit goals.

IV. Summary

In summarizing these studies we find:

(1) Uncontrolled factors are present in the usual form of instructions due to failure to make clear to the subject exactly what is called for in the statement of future goals. Thus, failure to understand instructions such as "What score will you get next time?", leads some subjects to respond in terms of 'hope,' others in terms of actual expectation, and still others in terms of a compromise of the two.

(2) When the performance level of others is known the level of the subject's own performance influences his difference score.

(3) Expressed goals (aspiration level) tend to regress toward the mean of performance of the group with which the individual identifies himself.

(4) The difference score for the same subject for several tasks shows some reliable consistency.

(5) Although several investigators have given slight attention to other variables, no significant experimental work has been done with any derived scores other than the difference score, the absolute level of aspiration and the absolute level of performance.

(6) Subjects with a long history of failure in a task differ from those with a history of success in the height and spread of their difference scores.

(7) All in all, in every level of aspiration situation the nature of the situational factors and certain individual 'personality' factors interact to determine the response. However, little or nothing is known about the stability or the nature of these personality factors.

(8) In order to determine the nature of these personality variables, it would be necessary to develop instructions that are not easily misinterpreted by the subject and in which previous experience with the task is controlled. It would also be necessary to examine all aspects of the individual's response, rather than to limit a description only to the difference between the expressed and achieved goals.

An attempt to develop such a method will be described in future papers.

BIBLIOGRAPHY

1. Adams, D. K. Age, race, and responsiveness of levels of aspiration to success and failure. (Abstract.) *Psychol. Bull.*, 1939, 36, 573.
2. Adler, A. *Problems of neurosis.* New York: Cosmopolitan, 1930.
3. Anderson, H. A., & Brandt, H. F. A study of motivation, involving self-announced goals of fifth-grade children and the concept of level of aspiration. *J. soc. Psychol.*, 1939, 10, 209–232.
4. Barker, R., Dembo, T., & Lewin, K. Frustration and regression: An experiment with young children. *Univ. of Iowa Stud. Child Welf.*, 1941, 18, No. 1.
5. Brown, J. F., & Rapaport, D. The role of the psychologist in the psychiatric clinic. *Bull. Menninger Clinic*, 1941, 5, 75–84.
6. Chapman, D. W., & Volkmann, J. A social determinant of the level of aspiration. *J. abnorm. (soc.) Psychol.*, 1939, 34, 225–238.
7. Clark, P., & Rice, C. A comparison of grades in college science with the students' own estimates. *Sch. & Soc.*, 1939, 50, 447–448.
8. Dembo, T. Anger as a dynamic problem. *Psychol. Forsch.*, 1931, 15, 1–144.
9. Dollard, J., Doob, L. W., Miller, N. E., Mowrer, O. H., & Sears, R. R. *Frustration and aggression.* New Haven: Yale University Press, 1939.
10. Doob, L. W., & Sears, R. R. Factors determining substitution behavior and the overt expression of aggression. *J. abnorm. (soc.) Psychol.*, 1939, 34, 293–313.
11. Escalona, S. K. The effect of success and failure upon the level of aspiration and behavior in manic-depressive psychoses. *Univ. of Iowa Stud. Child Welf.*, 1940, 16, No. 3, 197–302.
12. Fajans, S. Erfolg, Ausdeuer, und Activitat beim Saugling und Kleinkind. *Psychol. Forsch.*, 1933, 17, 268–305.
13. Frank, J. D. Some psychological determinants of the level of aspiration. *Amer. J. Psychol.*, 1935, 47, 285–293.
14. ——. The influence of the level of performance in one task on the level of aspiration in another. *J. exp. Psychol.*, 1935, 18, 159–171.
15. ——. A comparison between certain properties of the level of aspiration and random guessing. *J. Psychol.*, 1936, 3, 43–62.
16. ——. Individual differences in certain aspects of the level of aspiration. *Amer. J. Psychol.*, 1935, 47, 119–126.
17. ——. Recent studies of the level of aspiration. *Psychol. Bull.*, 1941, 38, 218–226.
18. Gardner, J. W. The relation of certain personality variables to level of aspiration. *J. Psychol.*, 1940, 9, 191–206.
19. ——. Level of aspiration in response to a prearranged sequence of scores. *J. exp. Psychol.*, 1939, 25, 601–621.
20. ——. The use of the term 'level of aspiration.' Psychol. Rev., 1940, 47, 59–68.
21. Gottschaldt, K. Level of aspiration in feebleminded children. *Ber. d. v. Kong. f. Heilpad, Munchen*, 1931, 130–143.
22. Gould, R. An experimental analysis of 'level of aspiration.' *Genet. Psychol. Monogr.*, 1939, 21, 1–116.
23. ——. Factors underlying expressed 'level of aspiration.' *J. Psychol.*, 1938, 6, 265–279.
24. ——, & Kaplan, N. The relationship of "level of aspiration" to academic and personality factors. *J. soc. Psychol.*, 1940, 11, 31–40.
25. ——, & Lewis, H. B. An experimental investigation of changes in the meaning of level of aspiration. *J. exp. Psychol.*, 1940, 27, 422–438.
26. Hausmann, M. F. A test to evaluate some personality traits. *J. gen. Psychol.*, 1933, 9, 179–189.

27. Hertzman, M., & Festinger, L. Shifts in explicit goals in a level of aspiration experiment. *J. exp. Psychol.*, 1940, 27, 435–452.
28. Hilgard, E. R., Sait, E. M., & Margaret, B. A. Level of aspiration as affected by relative standing in an experimental social group. *J. exp. Psychol.*, 1940, 27, 411–421.
29. Hoppe, F. Erfolg und Misserfolg. *Psychol. Forsch.*, 1930, 14, 1–62.
30. Horney, K. *New ways in psychoanalysis.* New York: Norton, 1939.
31. Jucknat, M. Performance and level of aspiration. *Psychol. Forsch.*, 1937, 22, 89–177.
32. Lewin, K. *A dynamic theory of personality.* New York: McGraw-Hill, 1936.
33. Lurie, W. A. Estimating the level of vocational aspiration. *J. soc. Psychol.*, 1939, 10, 467–473.
34. McGehee, W. Judgment and level of aspiration. *J. gen. Psychol.*, 1940, 22, 3–15.
35. Meerovitch, R. I., & Kandaratzkaya, K. M. The level of aspiration in hysterical children. *Trud. Inst. Izuch. Mozga Behkt.*, 1936, 5, 162–183. (Abstracted in *Psychol. Abstr.*, 1936, No. 4731.)
36. Radlo, G. Level of aspiration of schizophrenics. Worcester State Hospital. Unpublished.
37. Rosenzweig, S. Preferences in the repetition of successful and unsuccessful activities as a function of age and personality. *J. genet. Psychol.*, 1933, 42, 423–441.
38. Rotter, J. B. Level of aspiration as a controlled method of personality study using selected groups. (Abstract), *Psychol. Bull.*, 1941, 38, 587.
39. ——. Level of aspiration as a controlled method of personality study. (Abstract), *Psychol. Bull.*, 1940, 37, 518.
40. ——, & Rodnick, E. H. A study of the reactions to experimentally induced frustration. (Abstract), *Psychol. Bull.*, 1940, 37, 577.
41. Ryans, D. G. The measurement of persistence. *Psychol. Bull.*, 1939, 36, 715–759.
42. Sears, P. S. Levels of aspiration in academically successful and unsuccessful children. *J. abnorm. (soc.) Psychol.*, 1940, 35, 498–536.
43. Wickert, F. A test for personal goal values. *J. soc. Psychol.*, 1940, 11, 259–274.

[MS. received February 26, 1942]

SEMIOTIC AND PSYCHOLOGICAL CONCEPTS

BY L. O. KATTSOFF AND J. THIBAUT

University of North Carolina

I. The Nature of Semiotic

The problem of the analysis of concepts is basic to any science. If we are in the field of theory, this analysis is for the most part deductive; if we are interested in the verification of our analysis, we concern ourselves with experimentation. It is a trite remark that both aspects of the analysis of concepts are important—yet the triteness of the remark has not prevented undue emphasis upon one or the other aspect. These two aspects of the scientific analysis of concepts for a long time beclouded a third aspect which is important in the study of the meaning of words—this is the relation of the sign to the user. It is the purpose of this paper to discuss the theory of semiotic and to apply the discussion to some psychological concepts. In this discussion we shall be able to point out the methodological errors made both by naive experimentalists and pure operationalists—which are based upon a confusion of spheres of semiotic.

Semiotic is defined as the science of signs. Its locus is, to use an expression of Russell's, the realm of knowledge by description as contrasted with knowledge by acquaintance. Since scientific knowledge is acquired to a large degree through the process of symbolization, the role and properties of signs are extremely important. If we realize further that a sign is anything which denotes or means something to someone, then the importance of semiotic is extended to all spheres of human activity.

Scientific education and scientific research consist to a great degree in the construction and transmission of a set of symbols. One aspect of these symbols, namely their referents, has been of interest to scientists concerned with method in recent years. In fact, the scientist has come to turn more and

more to a reconsideration of his use of symbols. This is evidenced in the work of Hull from the point of view of what we shall call syntax; in the work of C. C. Pratt from the point of view of semantics; and in the confusion of many naive experimentalists who confound what we shall call pragmatics with semantics and call their confusion 'operationalism.' To a large extent, Tolman's work is pragmatic rather than semantic.

These remarks indicate the three branches of semiotic (6). The divisions of semiotic correspond to the relations that exist (1) between the sign and other signs, (2) between the sign and its referent, and (3) between the sign and the user of that sign. The theory of the relation between the sign and other signs is known as *syntactics*. The theory of the relation between the sign and its referent is called *semantics*, and that between the sign and the user of the sign is called *pragmatics*.

Syntactics deals with the relations of signs to each other. As a result, syntactics is not concerned with the extra-formal meanings assigned to the signs. Thus we shall call a system a syntactical system (or formal system) if the relations between its signs are not dependent upon the meaning of the signs but upon their formal aspect. The best illustration of a syntactical system is the system of geometry, *i.e.*, a postulational system.[1] One of the chief relations between signs which is found in the realm of syntactics is that of *inference*. Here we are concerned with that formal relation between signs by which it is possible to pass from one sign (or set of signs) to another. The construction of mathematical physics is an illuminating example of the development of a syntactical system. As a matter of fact the insistence upon quantification in other sciences seems to be based upon a feeling that mathematics alone makes possible the construction of a syntactical system.

The advantages of a syntactical system seem to be twofold: (1) it makes possible the calculation by mechanical rules of sign-combinations which lend themselves to interpretation and verification, (2) it tends to eliminate confusions due to the ambiguities of words in ordinary discourse.

[1] In psychology, Hull has tried to establish a syntactical system in his articles and in the book *A mathematico-deductive theory of rote learning*.

Syntactical operations are an intermediate stage between observation and formulation of data and the verification of new propositions about that data. Herein, however, there lies a difficulty and one which takes us out of the realm of syntactics.

Syntactical systems may be constructed from many points of view. One can construct a syntactical system based on rules of combinations arbitrarily defined, and symbols taken at random, independent of any possible reference to existential situations. Again a syntactical system may be constructed by setting up a set of symbols which represent given existential situations and the rules of combination of these symbols defined with reference to those situations. Calculation on the basis of these rules can then proceed in abstraction from the referents of the symbols. This is what happens in statistical analysis of sets of data and in the attempts at geometric interpretations of equations derived from sets of data.

But the derivation by calculation of the consequences of the initial symbols or by the construction of new sets of symbols from the old (*i.e.*, by syntactical definition) does not in itself guarantee that these latter have existential referents. To say the same thing in other terms, the problem of the interpretation and verification of the new sentences and symbols remains to be solved. The problem of semantics, then, is to determine whether or not the sign has a designatum and, if so, the relation between the sign and its designatum. This problem is illustrated in the application of matrices to the analysis of psychological and sociological data (= factor analysis). The derivation of the so-called 'factors' by the manipulation of matrices leaves the investigator with the problem of the interpretation, *i.e.*, the referents or meaning of these factors. If the authors understand the situation in factor analysis correctly, there is at present no generally accepted semantical rule, *i.e.*, no rule which determines under which conditions a factor-sign can be said to mean some property, ability, or what-not.

As previously stated pragmatics is defined as the theory which dealt with the relation between the sign and the user of

that sign. It will be clear throughout this paper that pragmatics may have that meaning in either an immediate or a mediate sense. In the first case pragmatics will represent the relation between the sign and the sign-user (who is constantly going out beyond the sign to construct a putative or ostensible referent) as a process which may or may not be interesting. In the mediate sense pragmatics becomes that specific division of semantics which inspects pragmatical relations in their immediate sense. Thus the pragmatical behavior of rats is simply the subject-matter (immediate) about which the interested experimenter is concerned to set up semantical rules (mediate). Or, again, what is pragmatic for the psychopath is pragmatic in a much different sense for the interested psychoanalyst.

Experimentation is basically a process for the determination and specification of the semantic aspect of theoretical structures. The aim of experimental precision is to eliminate accidental features of this semantical meaning and, even more often, supernumerary symbols in the scientific vocabulary. Thus, in the passage from the Law of Effect to the Law of Emphasis, those signs whose referents proved to be purely putative or mythical, were expelled from the language. In other words, pragmatic propositions without semantic correlates are eliminated. The experimentalist approaches any specific semantical area in piece-meal fashion. He brings with him (1) a number of independently derived semantical propositions and (2) a pragmatic knowledge of the general meaning of the events in the area. With these propositions and knowledge as evidence the experimentalist can by the method of inference (a syntactical operation) predict with some assurance the locus of a crucial problem. Still on a pragmatical basis, he can formulate a set of hypotheses about this problem which, if confirmed, becomes a set of semantical rules.

In other words, an experiment actually amounts to the semantical rule for a given sentence. This is an important fact because at this point lies the value of operationalism and the place where naive operationalists are misled. When the operationalist is compelled by criticism of his definitions of

concepts to assert that he means by this concept the particular operations he has performed or the particular types of behavior he has observed, then he begins to leave semantics for pragmatics. And this migration constitutes an evasion of responsibility which, if actually concluded, would leave us with a solipsism of ineffable operations.

It is possible for the pragmatic and semantic aspects of a sign to be identical, and the purpose of scientific procedure and sound education would seem to be, in many cases, to bring about this identification. Similarly, the propagandist and the demagogue would attempt to keep the two separate. So far as social behavior in general is concerned, pragmatics is at present of much greater significance than semantics. This is clearly illustrated in the use of the concept 'war.' Semantically 'war' has a definite meaning and refers to the use of armed force with more or less clearly understood effects upon the economic, social, and political structure of the state and upon the emotional, physical, and intellectual structure of the individual. Pragmatically 'war' means to some a horrible physical suffering; to others, an opportunity to display their patriotism; and to still others, an opportunity to destroy fascism, etc.

II. Semiotic Applications to Psychological Concepts

A great deal of the confusion which results from any attempt to define concepts arises from a lack of clarity of these realms of semiotic. That the definition of psychological concepts is especially difficult is due to the inability of scientists to keep distinct the semantic and the pragmatic meaning of the concept involved. Operationalism is an attempt to give a precise technique for the determination of semantic meanings. Thus it attempts to define a concept by means of 'operations' which can be repeated by any individual. The confusion in operationalism occurs because the operations that were designed to specify the semantic meaning of the concept actually tend to specify the pragmatical meaning. This tendency derives from the failure to distinguish between operations that *are* performed and operations that *should be* performed.

Operationalism tends to overlook the fact that the semantical meaning of a concept, however known, is always the limit towards which approximations approach. Thus operationalism fails to explain the modification and even radical alteration of the operations performed.

It is true that the observer cannot be wholly divorced from his subject-matter. The pragmatical relation must be constantly viewed as related to the semantical. Just as pragmatics involves a putative referent, so does semantics involve, in some sense, the spectator, and since a simple statistical generalization of pragmatical attitudes could result only in the projection and magnification of error, it is necessary to postulate an ideal observer as a semantical concomitant. This, however, does not imply what is usually taken to be operationalism. For, in the first place, the ideal observer is in fact no more than a list of specifications and qualifications (drawn up from semantical rules) toward which actual observers approach asymptotically. And second, the fixation of meaning at the level of actual (as opposed to normative) operations would involve the acceptance of a statistical description of the observer and the ignorance of the prescriptive function of the ideal observer.

Operations cannot be meanings in any case, although they do serve as important semantical rules for determining the meanings of signs for concepts. One of the most stupid remarks made by a scientist is when he retorts to questions concerning his definitions by the comment: "I am going to mean by 'x' that type of behavior 'y.'" Here is a palpable confusion of pragmatics and semantics and an unwitting attempt to hide a deficiency in semantical definition by pragmatical considerations. This is unworthy of any scientist! All experimentation becomes futile if such an attitude is adopted since no modification of that behavior is possible which yet retains the definiendum. Even worse, the failure to recognize the confusion and the subsequent enforced coincidence of the semantical meaning with the pragmatical one makes impossible further analysis and progress toward the semantical meaning—which, after all, is what the scientist is after.

The importance of keeping the distinction between pragmatics, syntactics, and semantics in view, is evidenced by an examination of the procedure that even an operationalist follows in the definition of psychological concepts. When, for example, the concept of 'intelligence' or 'insight' or even 'purpose' is under consideration, the scientist approaches his problem with some notions of what these concepts mean to him. This is to say that every investigation is based on some pragmatical meaning of the concept involved. Situations are constructed and observations made of more or less controlled data in an attempt to determine some *semantical* rule. Syntactical operations are performed both upon the pragmatical propositions and upon the acquired semantical propositions. Additional observation of the semantic meaning of the syntactical propositions is then necessary to determine whether there is agreement or disagreement between these syntactical propositions and the propositions syntactically derived from semantical propositions.

It becomes clear that there are three kinds of operations—syntactical, pragmatical, and semantical. Syntactical operations consist of those operations involved in drawing inferences, and include such operations as the arithmetic operations, the combination and reduction of matrices, etc. Pragmatical operations involve behavioral reactions to signs such as pointing, flight, weeping, etc. Unfortunately, semantical operations cannot be so easily enumerated—but experimental observations and techniques are one important type of semantical operations.

It would seem that semiotic operations are embedded in Tolman's 'means-end-readiness' and Krechevsky's 'hypotheses.' Especially clear is their presence when 'means-end-readiness' is taken flexibly as a more comprehensive and more general aspect of 'hypothesis.' In a jointly-written paper Tolman and Krechevsky write with regard to Krechevsky's earlier work (1, 2, 3, 4, 5):

> Krechevsky's rats when first put in the discrimination-box spent considerable time in trying to climb through the wires, in examining

the restraining wall, and the like, as well as in running in the alleys proper. . . . They exhibited, in short, a very general selectivity of the general *exploratory* variety, and this we shall now designate as the means-end-readiness. But within this . . . they also exhibited these various more specific sub-varieties of which running in the alleys was . . . most successful in reaching food. Such running in the alleys we shall designate therefore as one of a group of alternative hypotheses all of which appeared within the initial more general means-end-readiness of exploring. The first stage of learning seemed to consist in limiting the general means-end-readiness (to explore) to this single more specific hypothesis (to run in alleys).

Next, however, it appeared that the animal began to run not in any alley but rather to select all the right-hand alleys only, or all the left-hand alleys or alternate right and left alleys, and the like. At this stage we shall designate, therefore, the general running of alleys (instead of trying to climb through the wires, etc.) as the means-end-readiness; while these more specific patterns of choosing all right alleys, or all left alleys, etc., we shall designate as hypotheses. Finally, the last stage of learning seemed to consist in the selection of one of these subordinate hypotheses (running in the lighted alleys) and the reduction of the more general means-end-readiness (to run in any alley) to this single more specific form (7, p. 66).

It is our contention that, stated in this fashion, the learning process involves a general semiotic. First of all it is clear that only some of the events in the rat's environment are interesting and hence potential symbols. Second, some of these events seem to be syntactically (or formally) related, *i.e.*, there seems to be some recognition of the polarity and mutual relevance of such alternatives as right and left, up and down, dark and light. Evidence for this modality of response comes from Krechevsky's finding that 'maze-bright' rats preferred spatial hypotheses, while the 'maze-dull' rats preferred non-spatial hypotheses (4). True, a rat in a discrimination box finds his activity and alternatives sharply circumscribed. Krechevsky's apparatus, however, overcomes a great deal of this artificial legislation. It is also true that the *origin* of these formal relations is either molar or molecular (probably for the most part the former). But it need not be pointed out that any insistence on reduction at this point would introduce some form of the genetic fallacy.

Means-end-readiness then consists of a selectivity of potentially symbolic events and a syntactical ordering of these events. Marginal to specific responses, means-end-readiness is a general orientation and presumption of relevance. The pragmatic relationship enters most strikingly with the appearance of hypothesis. It might be said that any specific hypothesis is an assigning of presumptive reference to this particular pattern of events (which have now become actual symbols). When any hypothesis is verified, *i.e.*, when one particular set of pragmatic relations leads the rat to the food-box, that hypothesis becomes a semantical rule. It tells the rat something about the physical world.

The difficulties of typification and the whole theory of types are bound up in the theory of semiotic. It will be noticed that most typologies are based upon pragmatical meanings for which personality characteristics are sought. Spranger's five types are almost self-evident examples of a pragmatical typology. Typologies based upon the observation of psychopathic individuals and their physical characteristics come closer to being semantical in nature due to the basic relation between mental pathology and physiological functioning. If a genuine typology is to be developed, it will be necessary first of all to construct the type criteria in such a way as to reduce the pragmatical element to a minimum. This would seem to mean that the relation between symbol and referent must be defined in terms of overt behavior patterns other than linguistic ones.

Closely allied to the problem of types is that of stereotypes. A stereotype is the meaning which a given group name arouses in the minds of individuals. A stereotype carries, therefore, basically a pragmatical meaning. Stereotypes are fundamental in race prejudices. The recognition of their pragmatical character explains the difficulty of overcoming race prejudices by means of semantical rules. The assumption that has been made by many social psychologists that acquaintance with instances of the stereotyped class would break down the stereotypes, may actually be unfounded from this point of view.

Distinctions of pragmatical, semantical, and syntactical areas are necessary in the entire testing program. These distinctions must be made from two points of view. In the first place we cannot accept such statements as "Intelligence is what the intelligence tests test." Here the confusion is between a syntactical and a semantical definition. If 'intelligence' is defined in this way, then clearly no modification of the definition is permissible. No reference is intended by the definer to anything beyond the particular test, and therefore, to modify the test in any way is to treat of something different. From such a point of view any attempt to improve a test or to alter it, is a meaningless operation. Tests are obviously instruments and are to be recognized as such. They function as provisional semantical rules, subject to modification.

In the second place there is a tendency on the part of a test to confound the subject's ideas of what a particular word means to him, with that to which the word refers. This seems to be particularly true in vocational aptitude tests where the subject will tend to have visions about particular vocations. In some cases the pragmatical angle is the important one.

The implications of semiotic on general educational practice are also quite clear. It is necessary first to utilize the rather vague pragmatical notions which the student brings with him, in order to develop the broad contours of this or that science (or art). From an understanding of the general function, coherence, and context of (say) sociological statistics, the student must be taught the specific semantical propositions which are the elements of this science. The process is one of individuation.

These comments on the distinctions involved in semiotic and a few of their implications for psychological concepts may throw some light on a number of the problems generated by psychological methodology. Most of these suggestions will need experimental verification. If any investigation is stimulated by these remarks, they would appear to be of value if for that reason alone.

BIBLIOGRAPHY

1. KRECHEVSKY, I. 'Hypothesis' vs. 'chance' in the pre-solution period in sensory discrimination-learning. *Univ. Calif. Publ. Psychol.*, 1932, 6, 27–44.
2. ——. The genesis of 'hypotheses' in rats. *Univ. Calif. Publ. Psychol.*, 1932, 6, 45–64.
3. ——. The docile nature of 'hypotheses.' *J. comp. Psychol.*, 1933, 15, 429–443.
4. ——. Hereditary nature of 'hypotheses.' *J. comp. Psychol.*, 1933, 16, 99–116.
5. ——. A study of the continuity of the problem-solving process. PSYCHOL. REV., 1938, 45, 107–133.
6. MORRIS, C. W. Foundations of the theory of signs. *Int. Encycl. of Unified Science*, Vol. 1, No. 2, Chicago.
7. TOLMAN, E. C., & KRECHEVSKY, I. Means-end-readiness and hypothesis—a contribution to comparative psychology. PSYCHOL. REV., 1933, 40, 60–70.

[MS. received February 16, 1942]

THE IF-THEN RELATION AND SCIENTIFIC INFERENCE

BY JAMES A. CHRISTENSON, JR.

Duke University

In a recent article Johnson (4) has referred to the relation of implication and applies it to his definition of causal explanation in psychology. The present article is designed to show certain analytical properties of that relation, and to demonstrate their rôle in scientific inference. For comparison, I shall take the if-then relation (symbolized as $\rightarrow$) and its converse ($\succ$), both non-symmetrical, and the symmetrical relation of mutual implication ($\leftrightarrow$), here considered as a special combination of the earlier relations. Logically, each relation may be characterized in terms of truth-functions, which indicate the possibilities of transformation more clearly than definitions in terms of other relations:

	$P \rightarrow Q$	$Q \succ P$	$P \leftrightarrow Q$
(1)	$T\ T\ T$	$T\ T\ T$	$T\ T\ T$
(2)	$(T\ T\ F)$	$T\ T\ F$	$(T\ T\ F)$
(3)	$F\ T\ T$	$(F\ T\ T)$	$(F\ T\ T)$
(4)	$F\ T\ F$	$F\ T\ F$	$F\ T\ F$
(5)	$(T\ F\ T)$	$(T\ F\ T)$	$(T\ F\ T)$
(6)	$T\ F\ F$	$(T\ F\ F)$	$T\ F\ F$
(7)	$(F\ F\ T)$	$F\ F\ T$	$F\ F\ T$
(8)	$(F\ F\ F)$	$(F\ F\ F)$	$(F\ F\ F)$

Each relation has four possibilities of truth-combination. They are indicated by the combinations not in parentheses. In establishing a given relation, all four possibilities must be considered. Formally, of course, the criteria are quite narrow. In scientific investigation, however, no general relation is ever fully established, and the problem becomes rather that of determining the extent to which a postulated relation holds

in a given context, or how well the data in hand may be comprehended under an adduced relation.

The mixture of formal and empirical terms or the use of empirical terms only cannot be expressed in terms of the conventions of pure or formal logic. However, the introduction of certain extra-formal considerations will permit a treatment which seems to me quite useful for scientific inference. First, I shall give certain limited interpretations to each of the formal relations already introduced. And second, I shall impose limitations upon their scope of application. Given these limitations, the major operations of scientific inference can be empirically performed without danger of undue complication or veiling of problems by tautological multiplication of analyses. It will be exceedingly helpful to the reader to refer constantly to the truth-tables, and to confine all manipulations to the bounds here indicated. I deliberately refrain from stating a number of limitations commonly imposed upon logical reasoning, *e.g.*, the tabu upon translating from proposition-contexts to event-contexts. The *P*'s and *Q*'s so far mentioned may be any things of a kind; their *T* and *F* relations may be in any relevant context. How one may translate from one language to another is explicitly stated. No limitations upon such translations are meant, and nothing additional is meant. Redefinitions of any terms here used, while valid perhaps, must be carefully scrutinized for introduction of meanings not intended or given here, or exclusion of those specifically desired and included.

(1) The if-then relation ($\rightarrow$) will be considered to be one of 'implication,' and translated further by 'if-then' or 'predicts.' The converse relation ($\succ$) will be considered to be one of 'presupposition,' and translatable alternatively as 'comprehended under' or 'entails.' The relation of mutual implication ($\leftrightarrow$) will be considered to be one of 'association,' and roughly translatable as 'is related to' or 'is correlated with.' When this relation is asserted between any two sets of events, it will often be statable as a coefficient of correlation. The common interpretation of equivalence will be

ruled out unless that equivalence is formally asserted by an act of definition. Any verbal rendering of the meanings of these relations is at best partial; if the core of similarity is sought by the reader, tagged to his satisfaction, and constantly checked with the appropriate truth-table, he may allow for this inadequacy.

When $A \rightarrow B$, and $B \succ A$, with the two A's assumed to be identical and the B's also assumed identical, the relation between A and B is that of material implication, as considered in Johnson's article; this is, of course, possible only by an act of definition. But, where $A \rightarrow B$, and $B \succ A'$, with A' roughly, but not formally identical to A, the process is that of inductive generalization, which is of very definite use in scientific inference. This case, and that of $B \succ A'$ and $A' \rightarrow B'$, are included in the $\leftrightarrow$ relation.

For clarity of reasoning, no two terms may be juxtaposed, nor can two relations be side by side. Thus $A \rightarrow B \leftrightarrow B' \succ A'$ might express the process of attempting to re-infer A, when A is asserted to imply B, and B' is both asserted and asserted to be virtually identical to B (the right-hand term of the two-way relation is also asserted), and to presuppose A', *i.e.*, B' will re-infer some A' which may be stated to imply it, or more strictly some B''.

(2) In the relation $P \leftrightarrow Q$, both terms are to be of the same generality. In $A \rightarrow B$, A is to be general. In $B \succ A'$, B is to be specific. In $A \rightarrow B$, the A is always hypothetical, *i.e.*, formally assumed, *e.g.*, in propositions; in $B \succ A'$, A' is empirical in its basis, *e.g.*, in an integration of events. A and A' may have the same generality; they may even be assumed to be equivalent. The difference is in the manner by which each is obtained. Given the limitations now stated, $A \rightarrow B$ is a general statement of the relation of 'deduction,' and $B \succ A'$ is a general statement of the relation of 'induction'; in these two relations, reasoning should always be assumed to proceed from left to right, with the left-hand term asserted, and the right-hand term inferred. It is by appropriate combinations of these two relations that scientific inference may be assumed to proceed.

A certain degree of latitude has been left in the application of the relations. Each term employed is to be capable of consideration as a single element, but it may have any degree of complexity. The term may apply to a single event or to an exceedingly involved situation; it may apply to the assumption of a single occurrence, or to a comprehensive hypothesis. By translation, *T* and *F* in the truth-function tabulation may be rendered as happens and fails (empirically), compatible and incompatible (formally, as within a hypothesis), as well as true and false. Above all, the symbols are designed to be understood in terms of specific referents, whether empirical data or hypothetical statements. The task of reducing science to linguistic analysis is best left to advocates of difficulty for difficulty's sake; I feel that the practicing scientist cannot afford to bog down in analyses of procedures which are epiphenomenal to his actual investigations. It is sufficient to state the manner in which inferences proceed from formal to empirical contexts, and to indicate the empirically made identifications which bridge doubtful steps in the reasoning.

When these or any other relations are employed in scientific inference, it must be remembered that there can never be complete certainty, not even concerning the laws of logic, for it is only in their application to data that they may be considered. And, on the other hand, it is clear that the data can never be fully defined (*cf.* 3). A relation or correlation may be stated as the end product of a process of experimental and-or statistical variation of conditions. When fully stated, it will include a description of the process employed, a summary of the data comprehended, some indication of the scope covered, and a measure of the accuracy with which all this has been done. The generalizations or 'laws' so stated may be rendered in terms of the three relations given above; a generalization will be an *A'*, and a law will be a statement of 'association' or 'implication' together with the *A* concerned.

As I stated above, *A* and *A'* may be obtained in either of two manners: as formally asserted hypotheses (*A*) from which may be deduced certain consequences, or as empirical sum-

maries (A') of known data. The former manner of treatment makes of A a universal statement which is negated by a single false or unfulfilled instance; every unassimilable fact then requires the forming of a new hypothesis. The second approach treats of A' as never fully established; thus, where it is assumed that $B \succ A'$, where fact B is false, A' is false or the relation does not hold, but when B is true A' may be either true or false, *i.e.*, holding or not holding. It should be noted, further, that for $A \rightarrow B$, A may have no determinate empirical relation to B, for to ensure that the relation hold, B must somehow be classified and delimited, as it were 'reduced' by abstraction to a compass which may be covered by A. Operational methods seek to accomplish this reduction systematically. The present problem is: Given any theoretical abstraction, how can we, first, interpret it in terms of concrete events, and second, give a full and unambiguous enough description of those events to determine, on the one hand which of them are relevant, and on the other hand, to determine when something 'significant' is still undescribed. When A is asserted as a universal, and given an arbitrary interpretation, for purposes of scientific systematization, the question reduces to that of its applicability, *i.e.*, to establishing its scope and some index to its validity within that scope.

The usual experimental program involves an attempt to establish the presence of some relation, or to validate or determine the presumptive validity of some generalization. Thus, instead of interpreting $P \leftrightarrow Q$ as $P \equiv Q$, we are usually limited to the statement PrQ, where 'r' is an index of association or correlation. When both terms are empirically determined, the procedure is straightforward. But when one term is not so determined, as when a given hypothesis is being tested, it becomes desirable to find another basis for establishing its relation to facts than that of correlation, or complete consistency with the logical matrix of truth-values. What, then, is the probability that a given hypothesis A applies to those facts B_1, B_2, etc. which have been assumed to be related to it? In terms of the truth-tables, how may

lines 2 and 3 be ruled out of consideration, or quantified in terms of lines 1 and 4? A simple method is to assign probability values to specific deduced instances of B (*i.e.*, B_1, B_2, etc.), 1 if true, 0 if false. Then let the probability (p) for A be given as the number of B's which are true, divided by the total number observed: $p = T/(T + F)$. Where a given set of phenomena are to be embraced by a generalization, the decision between generalizations will be in terms of compatibility of the observed instances with their deduced truth or falsity. Both procedures, deduction of new facts, and assimilation of old, are generally combined; the selection of items may thus gradually be controlled. This control over selection must, however, by asymptotic to the limits set by the size of the sample; thus the formula, for $n > 1$, becomes of the form: $p = \frac{T}{T+F} \pm \frac{1}{T+F}$, where T and F have each numerical values. The last part of this formula is not to be interpreted statistically as a measure of error, but as indicating the maximum possible variation. It should be noted as a corollary to the formula that the results have no empirical significance unless there are some false instances, *i.e.*, $n > T$, and hence $n > 1$. Such significance may be attached by assuming that p for n true instances will be somewhere between that with one false instance, *i.e.*, $n - 1$ true, and that with all true, *i.e.*, $p = 1 = T/(T + 0)$. The value of p is then determined by the statement, $1 \geq p \geq \frac{(n-1)T}{nT}$. Placing p midway between n and $n - 1$, as a mean, gives the statement, $p = \frac{nT + (n-1)T}{2nT}$, which becomes $(2n - 1)/2n$, and when amended to $p = \frac{2n-1}{2n} \pm \frac{1}{2n}$, gives an empirical value for any number of related instances. In its simplest case, "$p = \frac{2-1}{2} \pm \frac{1}{2}$, *i.e.*, $\frac{1}{2} + \frac{1}{2}$." Any unsupported assertion, *e.g.*, "The moon is made of green cheese," will have this probability value. If A and A' of my earlier argument be assumed to be quantified, their relation may be

paraphrased as follows: $A = A' \pm K$, where K is some dependent variable.

One further interpretation of the $B \succ A'$ relation may now be made. If B's be taken to be the observations made by an investigator of certain phenomena, a special case of this process will be the designating of A' terms as 'facts.' A spherical object (A'), *e.g.*, is an object which has a circular outline (B) from whatever angle it is viewed (repetition of B's). Given the B's, then, A' is determinate within the range just indicated. It will be seen that a 'fact' is just as complex and proximative in its determination as a relation (*cf.* p. 489, par. 2). Moreover, the B's from which a given A' derives may be chosen in a number of ways; in all of these there is an arbitrary classification, often based on earlier empirical generalizations, though possibly formalized and stated abstractly. The ultimate probability value of A' as determined by this process will be a measure of success in the discovery of satisfactory classificatory or defining operations, and in the marshalling of sufficient collateral evidence; a value less than that strongly expected will often indicate the failure to discover implicitly given and unnoticed B's.

It might be noted, in conclusion, that this analysis permits a reconciliation between the statement of traditional logic that affirming the consequent or denying the antecedent in an if-then argument results in a *non sequitur* (*cf.* 1, pp. 216–217), and the type of inference made in scientific reasoning. In the paragraph just above, I have illustrated the empirical modification of affirming the consequent. A parallel modification of denying the antecedent may be applied to the use of investigatory hypotheses. When techniques are varied to demonstrate the absence of those results which hold when a given hypothesis is true, the result is equivalent to denying the antecedent, and nothing may be inferred according to the traditional logic. Considered in terms of the probability analysis given above, and of the possibility of inductive generalizations of the form $B \succ A'$, it will be seen that although 'guiding' hypotheses of any sort are necessarily hard

to validate, they can be evaluated roughly by application of this analysis.[1]

REFERENCES

1. Adams, D. K. Note on method. Psychol. Rev., 1937, 44, 212–218.
2. Christenson, J. A., Jr. Coordinates of psychology: analytic methods applied to the forms of psychological theory. Psychol. Rev., 1941, 48, 60–72.
3. Henderson, L. J. An approximate definition of fact. *Univ. Calif. Publ. Phil.*, 1932, 14.
4. Johnson, H. M. Rival principles of causal explanation in psychology. Psychol. Rev., 1939, 46, 493–516.

[MS. received March 9, 1942]

[1] To the reader who has looked for specific reference to psychology and failed to find it, I may say that this lack is intentional. For one thing, if what has been said here may be applied to psychology it may also be applied to any other scientific discipline. For another thing, I have felt it preferable to leave out specific applications which might perhaps be questioned on grounds not relevant to the present argument. To those who are interested in seeing such an addition of psychological content to a general methodological structure, a further elaboration of the point of view associated with the present approach may be found in my article, 'Coordinates of psychology' (2).

'REMINISCENCE' IN THE STUDIES OF PROFESSOR ENGLISH AND HIS ASSOCIATES [1]

BY CLAUDE E. BUXTON

Northwestern University

Professor English and his associates have done several experiments in which *S*'s read a prose passage and immediately afterward take a recognition test on the passage. This test usually is of the true-false variety.[2] It contains two types of item. Half are verbatim (V, based on a nearly exact statement from the text) and half are summary (S, based on the content of a sentence or several sentences or even a paragraph). If the *S*'s are later re-tested (at anywhere from 1 to 60 days) the number of correctly answered V items seems to decrease, while the number of correctly answered S items seems to increase. The apparent rise in the retention curve for S items has been called reminiscence by the investigators. The present note considers in a critical way a half-dozen characteristics of the studies thus far published in the series, and is particularly concerned with the validity of the finding of reminiscence.

Test-retest practice effects.—All the studies thus far reported are based in the main upon an immediate test and one or more later tests, upon the same group of *S*'s (**2**, **3**, **4**, **5**, **6**, **7**). As Bunch (**1**) and Gray (**10**) have pointed out, a legitimate measure of reminiscence is not possible with this procedure because the immediate test acts as additional practice. Edwards and English (**2**) themselves used an experimental design which avoided this difficulty, when they gave both an immediate and a later test to one group and later tests to other groups which

[1] I am greatly indebted to Dr. Harold Gulliksen of the University of Chicago for statistical advice, and to Dr. E. T. Katzoff of my own department for statistical and other aid.

[2] In one study (**5**), multiple choice items were used. In earlier studies (**7**), complication items were added to the test on a given passage, but only the number of items correctly checked as true or false entered into the score.

had not had the immediate test. However, having emphasized the importance of the immediate test, they in this paper and others continue to depend in the main upon the test-retest data for their evidence of reminiscence.

In later papers (3, 6) they have measured reminiscence in terms of the pattern of right and/or wrong responses on two or more successive tests—thus only items which were missed on the first test can be reminisced. In this case the immediate test affords practice in marking a given item wrongly, and such practice should not increase the number of right answers at a later test. This procedure is at least superior to the previous measurements in terms of group retention-totals, but certain misapplications in its use will be discussed.

Review.—McGeoch (12) and Forlano and Brunschwig (9) have compared *S*'s who admitted reviewing and those who did not during the interval between tests. From their work it seems likely that review has a somewhat favorable effect on recall. Therefore, if review is present, the retention scores are not fully legitimate indicators of reminiscence. In the studies of English and his associates the possibility of review has been considered unimportant. Their experimental design does not permit control of review by keeping *S*'s occupied during the rest interval, but nevertheless it seems desirable for these investigators to determine by questionnaire or other methods just how much reviewing is done and what effect it has. The claim of reminiscence probably cannot be made until the experimental design is changed and review ruled out.

Although the studies cannot be considered conclusive until the effects of test-retest practice and review are evaluated, it seems rather clear that neither factor could be called upon to explain the difference in retention for V and S items. Most of the remaining discussion will deal with influences which might create the differential.

Transfer.—Another factor which has not been studied adequately is transfer to the test material from the school or college curriculum. In a control experiment reported by English, Welborn, and Killian (7), 30 *S*'s took a test on the passage studied by experimental *S*'s, but without having read

it. They took the same test 55 days later. Although the authors say that the 'small' increase in scores was true for V and S items to about the same degree, computation of the changes in median scores for this group by the present writer shows an increase of 37 per cent for S items and only 7 per cent for V items. The subject-matter studied by both the experimental and control groups was of a psychological nature, and the *S*'s were general psychology students. (This was true in all but two of the studies: 3, 5.) It therefore seems plausible that what the authors have called reminiscence in the S items might be a transfer effect. According to this view, the small gain for the very specific V items, as a transfer from the general psychology course, is likely to be offset by forgetting, but since the S items were supposed to cover the content of a fairly large unit of subject matter, one might expect transfer to favor them more than the V items.[8] The S scores would be increased proportionately more by transfer and the gain might fail to be offset by forgetting, if forgetting occurred to the same extent as for V items. The differential trends are thus accounted for.

In two later researches (3, 5) the opportunity for transfer was lessened by the use of unfamiliar folk-tales or other learning material with school children, who probably did not have access to these or similar materials in the regular course of events. In these experiments there was no reminiscence in terms of group totals, which was the measure showing gains in previous experiments. There still was a slight superiority of S items in that they did not lose as much as V items. (The transfer explanation applies to this type of finding as well as to the actual appearance of reminiscence.)

Difficulty of items and absolute vs. relative measures.—One persistent source of trouble in the experiments, which has been recognized by English and Edwards (3, 6), has been the fact

[8] A control group similar to the one discussed above was used in an experiment by Edwards and English (2). It showed little or no change from first test to second. The time interval between tests was about half that in the group first cited, and not so much change *could* be expected.

It will be noted that not even the data on which the transfer explanation is based are free of test-retest or review effects.

that S items were much harder than V items, in terms of the proportion of correct responses on the first test. As has been mentioned, control groups who did not read the passage took the test in several experiments. In one rather exceptional case (7) such a control group scored nearly as high as experimental *S*'s on the S part of the test. In another (2), the mean S score of the controls (Fig. 1, p. 373) seems at least equal to the score of the experimental groups on the initial test. Such occurrences make it seem as though the S items are difficult for experimental groups because the answering of these items does not depend very much upon the previous reading. At any rate, if V items are mostly correct in experimental groups at the first test, the probable direction of change (reversals on two-alternative items) thereafter is downward, or a decrease in retention-score, while if S items are more often wrong on the initial test, they offer more opportunity for reminiscence—*i.e.*, an increase in retention-score. This type of reasoning will be used in the following detailed analysis of the paper by English and Edwards (6).[4]

In Table 4 of that paper (6, p. 259) the average number of persons having a given pattern of responses on any item (*e.g.*, RWR: correct on the initial test, incorrect on the second, and correct on the third) is shown for each quartile of difficulty. These quartiles are based on the number of *S*'s getting each item right on the initial test, when S and V items are thrown together.[5] When the occurrence of forgetting (RRW and RWW) or reminiscence (WWR and WRR) is tabulated against type of item (S or V), the authors apparently show (Table 5, p. 261) that reminiscence tends to occur more often with S items and forgetting more often with V items, with difficulty 'ruled out' by the special type of summation employed in Table 4.

However, the type of analysis they have made overlooks a

[4] The data they discuss in this paper are taken from an earlier one (2). This was the instance mentioned previously, in which S scores, here elaborately analyzed, were initially no better for the experimental groups than for a control group which had not read the passage. Both groups scored slightly less than chance alone should have made possible.

[5] Of the S items 8 fall in the first quartile (greatest difficulty), 8 in the second, 3 in the third, and 1 in the fourth; of the V items, 2 are in the first quartile, 2 in the second, 7 in the third, and 9 in the fourth.

very important fact. The evaluation of amounts of reminiscence or forgetting in their data should be made in terms of the opportunity for either to occur, rather than simply in terms of its absolute number of occurrences. In their case the factor of difficulty is also involved because V items, relatively easier, offer more opportunity for forgetting than they do for reminiscence, and because S items, relatively more difficult, offer more opportunity for reminiscence than they do for forgetting.

Their Table 4 can be re-written in a form which provides data for chi-square analyses which test in an appropriate manner for the influence of either difficulty or type of item upon the occurrence of either reminiscence or forgetting. Below are shown the data for a test of the independence of

	D_1	D_2	D_3	D_4	Σ
Occurrence.....	63	50	10	2	125
Non-Occurrence....	116	59	12	0	187
Σ.....	179	109	22	2	312

difficulty and occurrence-non-occurrence of reminiscence in S items. The column totals show opportunity for reminiscence to occur (number of wrong responses on first test); the row totals show actual occurrence and non-occurrence. χ^2 for this table = 6.017, d.f. = 3, and P = .11. Therefore the null hypothesis, viz., that there is no relationship between the two variables, cannot be rejected with a great deal of confidence.

Below are shown the data for a similar analysis of reminiscence with V items. This time χ^2 = .859, d.f. = 3, and P = .83. The null hypothesis cannot be rejected.

	D_1	D_2	D_3	D_4	Σ
Occurrence.....	15	8	20	9	52
Non-Occurrence....	27	16	29	11	83
Σ.....	42	24	49	20	135

These two χ^2 may be summed (8) and the total χ^2 has 6 d.f. χ^2 now = 6.876 and P = .34. The null hypothesis, viz., that the occurrence or non-occurrence of reminiscence in the two

kinds of items is not related to difficulty, cannot be rejected. Thus far the conclusion of English and Edwards (6) is supported.

We can now re-write the tables presented above and test for the independence of type of item and occurrence-non-occurrence of reminiscence, within each difficulty quartile *separately*. In each of the tables shown d.f. = 1; row totals

	D_1				D_2		
	Oc.	Non-Oc.	Σ		Oc.	Non-Oc.	Σ
S	63	116	179	S	50	59	109
V	15	27	42	V	8	16	24
Σ	78	143	221	Σ	58	75	133
	D_3				D_4		
	Oc.	Non-Oc.	Σ		Oc.	Non-Oc.	Σ
S	10	12	22	S	2	0	2
V	20	29	49	V	9	11	20
Σ	30	41	71	Σ	11	11	22

now indicate opportunity and column totals the actual occurrence and non-occurrence. The four χ^2 follow: D_1, .006, P = .93; D_2, 1.292, P = .26; D_3, .133, P = .72; D_4, 2.200, P = .15. Again these four χ^2 may be combined; the total $\chi^2 = 3.631$, d.f. = 4, and P = .46. Hence, contrary to the conclusion of English and Edwards, the null hypothesis, viz., that the occurrence or non-occurrence of reminiscence is not related to type of item, is sustained.

We shall show next the test for independence of difficulty and occurrence-non-occurrence of forgetting. In the table below, S items only are recorded. Again column totals show

	D_1	D_3	D_2	D_4	Σ
Occurrence.....	25	33	20	2	80
Non-Occurrence....	36	98	48	26	208
Σ.....	61	131	68	28	288

opportunity for forgetting and row totals the actual occurrence and non-occurrence. $\chi^2 = 11.877$, d.f. = 3, and P = .01.

We may reject the null hypothesis, viz., that the two variables are not related. Conversely, the discrepancies in the table are so large that occurrence or non-occurrence of forgetting *does* depend upon difficulty in S items.

In the table below we shall test for the independence

	D_1	D_2	D_3	D_4	Σ
Occurrence	8	15	46	54	123
Non-Occurrence	10	21	115	196	342
Σ	18	36	161	250	465

of occurrence-non-occurrence of forgetting and difficulty. $\chi^2 = 10.615$, d.f. = 3, P = .015. The conclusion just cited for S items is now seen to hold also for V items. Combining the two χ^2, the total $\chi^2 = 22.492$, d.f. = 6, P = .01. Clearly, difficulty and occurrence-non-occurrence of forgetting are related, according to the data presented by English and Edwards.

Finally we shall present tests of the independence of type of item and occurrence-non-occurrence of forgetting, within each difficulty quartile. In each of the tables shown below, d.f. = 1. The interpretation of marginal totals now is: row sums show opportunity, column sums show actual occurrence

	D_1				D_2		
	Oc.	Non-Oc.	Σ		Oc.	Non-Oc.	Σ
S	25	36	61	S	33	98	131
V	8	10	18	V	15	21	36
Σ	33	46	79	Σ	48	119	167

	D_3				D_4		
	Oc.	Non-Oc.	Σ		Oc.	Non-Oc.	Σ
S	20	48	68	S	2	26	28
V	46	115	161	V	54	196	250
Σ	66	163	229	Σ	56	222	278

and non-occurrence. The respective χ^2's are: D_1, 1.849, P = .18; D_2, 3.828, P = .05; D_3, .015, P = .90; D_4, 3.215, P = .07. The total $\chi^2 = 8.907$, d.f. = 4, P = .07. Again, a conclusion of English and Edwards' must be reversed: we

cannot with very great confidence reject the null hypothesis, viz., that occurrence-non-occurrence of forgetting is not related to type of item.

The authors' conclusion (6, p. 262) is: ". . . that a definite correlation does exist between forgetting and whatever is specifically and differentially measured by the V-items, and between reminiscence and that which is differentially measured by the S-items. This association does not seem attributable to chance or to differences in the initial difficulty of the two kinds of items. We are thus back to the conclusion reached in previous studies." The present analysis shows, on the contrary, that when relative rather than absolute data are used, forgetting and reminiscence are both independent of type of item, reminiscence is independent of difficulty and forgetting is *not* independent of difficulty.

We may reasonably infer that, since in the original data S items were more difficult than V items, the relative amounts of forgetting should be influenced by this fact. But the use of absolute data alone obscures this possible cause of differential retention for the two types of items. It should also be emphasized that the original and most important conclusion of English and Edwards, about the dependence of occurrence of reminiscence upon type of item, seems to be refuted.

The above rather complicated discussion was necessary because the apparent differences in retention for the two kinds of item could not be evaluated without taking account of opportunity and thereby automatically taking account of difficulty as a factor. The pattern analysis, while it places a desirable emphasis on what happens to specific responses, has not taken into account, as it has been used thus far, the relation between the number of items showing reminiscence or forgetting patterns and the opportunity for either type of change to occur.

In a later paper of the series (3), the difficulty of S and V items was made very nearly equal on the initial test. Here the authors present data in terms of opportunity for reminiscence or forgetting, without, however, testing these data statistically. Instead, they analyze the absolute scores of their Table I (p. 104), and conclude (p. 105), "There is evidently

something about the V-items which is correlated with less reminiscence while the reverse is true of S-items." In the table below are shown their data for opportunity to forget and to reminisce, and the extent to which the opportunity was utilized. There is no difference between the two kinds of items with respect to forgetting, when difficulty is ruled out. There is a slight difference in amounts of reminiscence,

	V	S
Opportunity for forgetting	2432	2306
Occurrence	632	609
Per cent forgetting	26	26
Opportunity for reminiscence	1108	1234
Occurrence	454	563
Per cent reminiscence	41	46

which when tested with the appropriate estimate of its S.E. (11), yields a C.R. of .925. Because there is a larger absolute number of forgotten items than reminisced, the mean retention curve of both types of item trends downward.[6]

The point to be made by the re-analyses given above is that where difficulty of items is not controlled, it, rather than the type of item, tends to be a differential factor in the retention curves. When difficulty *is* controlled, there is no *mean* reminiscence in either type of item—in fact they both show a slow decline in retention.

Other criticisms.—Mention should be made of certain other weaknesses in the experiments or the reports of them.

(1) In only one or two instances are reliability coefficients given for the two types of test item separately, although the content of the tests was changed several times. Only one r between the two types of item is given (7) for a single test sitting, and in this case but one reliability coefficient is cited for the two kinds of item upon which the r is based. It seems possible, in considering the way the items are constructed, that the two kinds will correlate to the limit permitted by the

[6] If the raw data given in this paper are correct, the means cited by the authors for two tests 30 days apart on the two kinds of items are erroneous. The first test on V items should have a mean of 20.61 and the second, 19.10. The first test for S items should have a mean of 19.54 and the second 19.15—contrary to the claim of the authors that the S items show no change at all. The means for the two half-tests combined should be 40.15 and 38.25.

unreliability of the half-tests. Objective and repeatable differentiations between S and V items are greatly needed. Published illustrations have not been as clear to the present writer as they apparently were to English and his associates.

(2) Although *S*'s in each of the experiments studied a given passage and then had two types of test item on it, the authors recognize this point but part of the time, and at other times are given to making statements such as the following (**5**, p. 272): "The comparison, to be sure, is still only between two degrees of the influence of meaning upon the process of learning." Since the learning was a single process which took place before the test, such a statement could properly refer only to the effect of the immediate test upon later tests. Later in the same paper (p. 276) they say: "The divergence of retention trends . . . is a revelation (by means of these measuring instruments) of differentiated learning processes." Such a differentiation in learning processes is difficult to conceive, although two different reflections of a given process by two different measuring instruments seem quite possible.

(3) In Table I (**5**, p. 274), the difference between two means for retention of S multiple-choice items is erroneously entered as .3 when it should have been 1.3. What was an insignificant loss in the mean of S scores then becomes a nearly significant loss (C.R.$_{P.E.}$ = 3.47).

(4) It seems difficult or impossible ever to say just *how much* reminiscence is actually occurring with a two-alternative recognition test, because the proportion of *S*'s and the proportion of items showing reminiscence seem certain to be larger than would be found with recall measures. The definition of reminiscence should probably be based upon improvements in terms of a recall criterion.

Conclusions.—1. The divergent trends of retention for S items (content of a sentence or several sentences or a paragraph) and V items (verbatim) are found mainly or only where the difficulty of the two kinds of items is not controlled and where absolute rather than relative numbers of right and wrong responses (or response patterns) are used.

2. Reminiscence has not been demonstrated to occur to a greater extent in S items than in V items.

3. In the later studies of the series, where difficulty is ruled out and transfer seems less likely, there seems to be little group-total evidence of reminiscence, for the reversals in this direction are more than balanced by reversals in the direction of forgetting.

4. The long-term gains which have occurred cannot be called reminiscence until several experimental controls are applied, and until all the data are taken into account in making an analysis for the occurrence of the phenomenon.

REFERENCES

1. Bunch, M. E. The measurement of reminiscence. Psychol. Rev., 1938, 45, 525–531.
2. Edwards, A. L., & English, H. B. The effect of the immediate test on verbatim and summary retention. *Amer. J. Psychol.*, 1939, 52, 372–375.
3. ———. Reminiscence in relation to differential difficulty. *J. exp. Psychol.*, 1939, 25, 11–108.
4. English, H. B. Further data on reminiscence. *Psychol. Bull.*, 1935, 32, 688 (abstract).
5. ——, & Edwards, A. L. Studies in substance learning and retention: XI. The effect of maturity level on verbatim and summary retention. *J. gen. Psychol.*, 1939, 21, 271–276.
6. ———. Reminiscence, substance learning, and initial difficulty—a methodological study. Psychol. Rev., 1939, 46, 253–263.
7. ——, Welborn, E. L., & Killian, C. D. Studies in substance memorization. *J. gen. Psychol.*, 1934, 11, 233–260.
8. Fisher, R. A. *Statistical methods for research workers.* (Fourth Edition.) Edinburgh: Oliver and Boyd, 1932.
9. Forlano, G., & Brunschwig, L. Perseveration in relation to reminiscence and recall. *Brit. J. educ. Psychol.*, 1938, 8, 178–187.
10. Gray, S. The influence of methodology upon the measurement of reminiscence. *J. exp. Psychol.*, 1940, 27, 37–44.
11. Kenney, J. F. *Mathematics of statistics.* New York: Van Nostrand, 1939 (Part II, p. 119).
12. McGeoch, G. The conditions of reminiscence. *Amer. J. Psychol.*, 1935, 47, 65–89.

[MS. received December 31, 1941]

REMINISCENCE—REPLY TO DR. BUXTON'S CRITIQUE

BY HORACE B. ENGLISH

Ohio State University

Like the small child who prefers being spanked to being ignored, my co-authors and I are sincerely grateful to Dr. Buxton for his painstaking critique of our studies in substance learning.[1] We shall take up Dr. Buxton's criticism in roughly the order of the foregoing article. Tempting as it is, we shall omit answers to a number of relatively minor points. Science is unlikely to benefit greatly from a decision on points between Buxton and us. There seem to us and, we think, to Buxton two major issues.

The meaning of reminiscence and the experimental design.—Our studies did not begin in a search for reminiscence; we found, however, a rise in certain recognition-retention curves which so exactly conformed to what Ballard and after him others had called reminiscence that we adopted the term. Had we been content to present our data merely in terms of changes in averages (complete with critical ratios, of course), there would have been no controversy. It soon became apparent to us, however, that the conventional concept of reminiscence and much current analysis of data was confused; and we were led, step by step, to a strict experimental definition of the phenomenon. We found it necessary to reject a rise in averages as the criterion (but the rise in our studies is very real, not 'apparent' as Buxton says) because such an average rise confounds phenomena moving in opposite directions. We came at length to the definition of reminiscence as *recall or recognition of a specific memorandum* (*i.e.*, of an

[1] In preparing this reply, I am so greatly indebted to Dr. Allen L. Edwards and Professor Maurice C. Moggie that it would be entirely proper to list them as joint authors. In what follows it is the plural, not merely the editorial 'we' which is used. The final draft, however, is mine and I must, therefore, assume responsibility for any errors.

item to be remembered) *after earlier failure and without intervening re-presentation of the original memorandum.*

Buxton nowhere states what he means by reminiscence but he joins Bunch (2) and Gray (7) in denying the legitimacy of a test-retest measure of the phenomenon. We had insisted, on the contrary (in our latest article [6], which Buxton had apparently not seen), that only when we have initial failure followed by success is there any *phenomenon* for which the term reminiscence is needed. We challenge any critic to set up an experiment or an unambiguous example from daily life which does not really rest upon this concept. If, therefore, we give up the test-retest measure, we give up reminiscence as phenomenon altogether and make of it an untestable hypothesis.

And we insist that definition be not confused with explanation. The phenomenon defined as we define it may be explained as due to transfer, to physiological consolidation, or to chance; it remains a rather stubborn fact. We, ourselves, have never committed ourselves to any one explanation, believing that not enough is known of the conditions under which the phenomenon occurs. We have, however, recently shown (6) something of what it is *not* due to: only in Looking Glassland can reminiscence be attributed to the practice effect of the earlier test.

By the same token 'review, conscious or unconscious' is ruled out as explanation. Note what the specific facts are. One fails upon demand—social, practical, or, it may be, experimental—to recall or recognize certain memoranda. Later, upon another demand, but without intervening re-presentation of the memoranda, one is able to effect recall or recognition. To attribute the demanded recall or recognition to an earlier spontaneous recall called 'mental review' is not to explain but to deepen the mystery. For surely it is obvious that the 'mental review' involves that very making of a correct response after an earlier failure which constitutes the phenomenon under examination. We cannot explain the occurrence of a phenomenon by citing the fact that it occurs.

The confusion on this issue of rehearsal, we believe to be

at least partly due to thinking in terms of *average* gains or losses instead of in terms of specific psychological data—in this case the memoranda and their explicit recall or recognition. We may properly divide our data into three possible responses: right, wrong, or failure to respond. Rehearsing the correct response may lead to overlearning but surely not to reminiscence. And how can rehearsing a failure to respond (whatever that means), facilitate making the right response? How can rehearsing a wrong response help?[2] Yet that direction of change is what is necessary for reminiscence. Rehearsal thus does not explain the specific reminiscence change. Rehearsal of the already reminisced response, however, may help to prevent relapse; and it may also retard forgetting of the originally right responses. Thus, if there has already been reminiscence, rehearsal may help to insure that it shows up in the *average* without in the least explaining its actual occurrence.[3]

The transfer hypothesis advanced by Buxton seems to have somewhat more merit as applied to our data, though here again we should insist upon the difference between explaining and explaining away. Nor do we object in principle to Buxton's argument that our S-items may be more easily helped by transfer than our V-items. We suggest, however, that Buxton's speculation conforms less neatly to certain of our data (4) than to those chosen for his illustration.

We do challenge the dictum that reminiscence should be defined in terms of a recall criterion rather than recognition. To be sure no direct comparison of reminiscence as measured by the two criteria is possible, but why make one primary?

The differential trends in memory.—A more serious issue is raised by Buxton's criticism of our attempts to show differ-

[2] Since the passage learned has a certain unity, it is possible that rehearsal of the *whole* may, in fact, help to redintegrate certain members missing earlier. The hypothesis should be easy to test experimentally. It is unlikely to apply to or explain all reminiscence.

[3] We here include under 'reminisced response' as operationally defined those changes from wrong to right which are of pure chance character. Our explanation of the *average* gain is then a generalization of W. Brown's 'chance plus practice of chance success' explanation (1). We believe Brown's statement of the case entirely valid but probably inadequate to explain all the facts.

ential trends for the two kinds of items. Buxton is impressed —as we were—with the possibility that the differential trends in scores are due, not to the differences in the learning or retaining processes behind the test results but merely to differences in the difficulty of the test item. Hot on this scent he substantially duplicates our own destructive critisism of our earlier analyses. By an elaborate chi-square analysis he proves what seemed to us obvious from per cents alone (see **5**, Table 3), that difficulty of item at the time of first testing is not unrelated to the reminiscence-forgetting continuum.[4]

When we became convinced of this fact, our next step was to ask whether there is an association between our two continua (Reminiscence-Forgetting and the S- to V-type of item) which would be *independent* of the admitted association with difficulty (see **5**). Not unnaturally, we turned first to a 'statistical equation' for difficulty; and we found that even when allowance is made for the difference in difficulty, the association does not disappear.[5] Later, aware of the hazards of unnoted assumptions in any purely statistical equation, we sought to control the variable experimentally by constructing our tests in such manner that initial difficulty was the same; and still the association remained (see **3**). In all future work with reminiscence, it would now appear certain, due and careful account must be taken of the influence of difficulty upon the opportunity for reminiscence to occur. At this point Buxton, though he seems to think otherwise, has most vigorously reinforced our position.

Buxton has, indeed, performed a service in clearly isolating opportunity-to-occur as a key problem in the study of either forgetting or reminiscence. He proposes, however, a definition which we find ourselves unable wholly to accept. Es-

[4] Indeed it now seems to us formally and logically *necessary* that initially difficult items will, other things being equal, show more reminiscence than initially easy ones. Failure to find the association would therefore mean merely that other things were not equal.

[5] Professor Moggie points out, however, that in equating for difficulty we made the chi-square test of somewhat doubtful validity. We were compelled to assume that the error of corrected totals is proportionally the same as that of the observed numbers. While the difference is probably not great, it may not be negligible.

sentially he follows tradition in giving a purely formal or statistical meaning to 'opportunity-to-occur.' Here are certain items to be remembered; you forget a certain proportion of the total you might—formally—have forgotten, that is of those items which were once 'learned' well enough to yield an experimentally defined correct response. Similarly he defines opportunity to reminisce in terms of the proportion of all those items not learned well enough to yield a correct initial response.

This proposal at first seems eminently reasonable. We used precisely this analysis ourselves (5) but found it necessary later to reject it. We did so on the grounds which run like a theme song through all this work, that it confounds in one measure *psychologically* distinct responses.

Both forgetting and reminiscence are phenomena of partial or incomplete learning. Now we submit that when a person consistently answers an item correctly on tests taken months apart we are dealing with an 'overlearned' or 'completely learned' memorandum. Such a memorandum may *formally* offer opportunity to be forgotten; psychologically it does not—not, certainly, within the experimentally defined limits.

Similarly and even more obviously, when a person consistently answers items incorrectly, the best estimate we can make is that he never learned most of these items at all—none of them up to the experimentally defined criterion. Now from Plato on down, reminiscence has referred to that which comes *back*. Here the evidence is it was never here to come back. To include thoroughly learned items in the 'opportunity to forget' and never learned items in the 'opportunity to reminisce' is to load the statistical dice against finding either phenomenon. It is strictly analogous to listing all responses in an examination as 'honest' in which a person did not cheat, paying no heed to the fact that a person might know the answers and hence feel no need to cheat.

Of lesser importance but along the same line is Buxton's inclusion of such patterns as R W R and W R W in 'opportunity not taken advantage of.' Obviously both patterns ex-

hibit *both* reminiscence and forgetting as experimentally defined. To include such ambiguous cases in favor of either seemed to us too much like trying to make a case; to include them against the hypothesis seems equally overreaching.[6]

Rejection of Buxton's definition of opportunity-to-occur does not, however, prove that our own was correct. And, in fact, I am now ready to admit that our *exclusion* of the R R (or R R R) and W W (or W W W) patterns was not wholly legitimate.

The memoranda thus categorized in many cases certainly were not open in any but a formal sense to either forgetting or reminiscence. And we reject the purely formal. But on the other hand, it is only by resort to an arbitrary criterion that we can deny that some of them were open to such a fate. A memorandum which leads to W W W responses *may be* partially learned and hence subject to whatever it is that leads to reminiscence. And quite certainly, R R R can only be formally regarded as 'completely learned,' even when the last R is three months from the presentation. It is hardly consistent for me to reject the formal criterion as used by Buxton and utilize another, equally formal, myself.

Nor do I see any way immediately to resolve the dilemma. Our data and our analysis do, I think, actually establish a differentiation between our two kinds of test items—a differentiation soundly defined, formally. But when I come to interpret the differentiation, I am compelled to observe the limitations imposed by an arbitrary rejection of part of the data. Yet it will not do to include them, as Buxton does—equally arbitrarily and with less regard to their psychological significance.

[6] Of minor importance likewise is Buxton's technique in adding chi-squares. Chi-squares may be legitimately added only when the samples are analogously drawn to test a single hypothesis. Since Buxton and we have proved that levels of difficulty constitute a real factor, adding samples from different levels when testing for the reminiscence-forgetting hypothesis seems to violate one of the assumptions of the statistics.

The reasoning is complicated, as he himself says, but we believe that in one analysis Buxton has overdetermined the case by taking account of difficulty twice over, once when our equation for difficulty is accepted, again under the guise of opportunity to occur—which is a corrollary of difficulty. But our reply cuts under this particular issue to the basic one of how we are to measure 'opportunity to occur.'

In short, I should defend our experimental conclusion but admit that the experimental design has proved inadequate to the interpretation offered. We began with a highly conventional analysis using averages. This proved to hide ambiguities. We moved on to a pattern analysis which removed most of the ambiguities and which clarified the concept of reminiscence. *En route* we found reason to think that practically every other reported study of this problem is vitiated by failure to take account of similar ambiguities. If we persuade some of our colleagues to look at their data psychologically before they begin their smooth-functioning statistical routines, our trial-and-error advance will prove a real contribution. But, thanks to Dr. Buxton's critique, we arrive at the conclusion that we have not ourselves sufficiently mastered the problem of setting up categories which are at once unambiguous and meaningful. And without them, we have no secure interpretation. At the present time there seems to be no experimental design which enables us to make a real estimate of the proportion of reminiscence or forgetting, either in our data or any other.[7] For the time being I have no suggested solution but I maintain my faith that ambiguities can always be removed when the problem is clearly stated.

In summary, then, we reaffirm the soundness of our concept of reminiscence as a matter of the *individual* memorandum or item to be remembered which is first forgotten or falsified, then corrected. This concept is experimental, feasible, and congruent with usage but more explicit.

We welcome the sharpening of the concept in the case of both forgetting and reminiscence of 'opportunity to occur,' but reject Dr. Buxton's definition thereof. But prodded to it by his criticism, we find our own definition also faulty. Only an improved experimental design can resolve the difficulty.

REFERENCES

1. Brown, W. To what extent is memory measured by a single recall? *J. exp. Psychol.*, 1923, **6**, 377–382.

[7] This is not to say that we cannot unambiguously point to reminiscence phenomena.

2. Bunch, M. E. The measurement of reminiscence. Psychol. Rev., 1938, 45, 525–531.
3. Edwards, A. L., & English, H. B. Reminiscence in relation to differential difficulty. *J. exp. Psychol.*, 1939, 25, 100–108.
4. ———. The effect of the immediate test on verbatim and summary retention. *Amer. J. Psychol.*, 1939, 52, 372–375.
5. English, H. B., & Edwards, A. L. Reminiscence, substance learning, and initial difficulty—a methodological study. Psychol. Rev., 1939, 46, 253–263.
6. ——. Practice as cause of reminiscence. Psychol. Rev., 1941, 48, 524–529.
7. Gray, S. The influence of methodology upon the measurement of reminiscence. *J. exp. Psychol.*, 1940, 27, 37–44.

AMERICAN PSYCHOLOGICAL PERIODICALS

American Journal of Psychology—Ithaca, N. Y.; Cornell University. Subscription $6.50. 624 pages annually. Edited by K. M. Dallenbach, Madison Bentley, and E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Carl Murchison. Quarterly. Child behavior, animal behavior, and comparative psychology. Founded 1891.

Psychological Review—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $5.50. 540 pages annually. Edited by Herbert S. Langfeld. Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $6.00 per volume. 500 pages. Edited by John F. Dashiell. Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Bulletin—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $7.00. 665 pages annually. Edited by John E. Anderson. Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University. Subscription $6.00 per volume. 500 pages. Edited by R. S. Woodworth. Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $5.00. 560 pages annually. Edited by Gordon W. Allport. Quarterly. Founded 1906.

Journal of Educational Psychology—Baltimore, Md.; Warwick & York. Subscription $6.00. 720 pages annually. Edited by J. W. Dunlap, P. M. Symonds, and H. E. Jones. Monthly except June to August. Founded 1910.

Psychoanalytic Review—New York, N. Y.; 64 West 56th St. Subscription $6.00. 500 pages annually. Edited by Smith Ely Jelliffe. Quarterly. Founded 1913.

Journal of Experimental Psychology—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $14.00 per annum (2 volumes). 1040 pages annually. Edited by Samuel W. Fernberger. Monthly. Founded 1916.

Journal of Applied Psychology—Indianapolis, Ind.; C. E. Pauley & Co. Subscription $6.00. 600 pages annually. Edited by James P. Porter. Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Co. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Roy M. Dorcus, Knight Dunlap and Robert M. Yerkes. Bi-monthly. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; Williams & Wilkins Co. Subscription $6.00 per volume. 400 pages. Edited by Roy M. Dorcus. Without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Provincetown, Mass.; The Journal Press. Subscription $7.00. 500 pages annually. Edited by Carl Murchison. Bi-monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Northwestern University, Evanston, Illinois; American Psychological Association, Inc. Subscription $7.00. 700 pages annually. Edited by Walter S. Hunter and H. L. Ansbacher. Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 1000 pages annually. Edited by Carl Murchison. Quarterly. Experimental, theoretical, clinical, and historical psychology. Founded 1927.

Journal of Social Psychology—Provincetown, Mass.; The Journal Press. Subscription $7.00. 500 pages annually. Edited by John Dewey and Carl Murchison. Quarterly. Political, racial, and differential psychology. Founded 1929.

Psychoanalytic Quarterly—Albany, N. Y.; 372–374 Broadway. Subscription $6.00. 560 pages annually. Edited by Bertram D. Lewin and others. Quarterly. Founded 1932.

Character and Personality—Durham, N. C.; Duke University Press. Subscription $2.00. 340 pages annually. Edited by Karl Zener. Quarterly. Founded 1932.

Journal of Psychology—Provincetown, Mass.; The Journal Press. Subscription $14.00 per annum (2 volumes). 800–1200 pages annually. Edited by Carl Murchison. Quarterly. Founded 1936.

Psychometrika—University of Chicago, Chicago, Ill.; Psychometric Society. Subscription $10.00. 320 pages annually. Edited by L. L. Thurstone and others. Quarterly. Quantitative methods in psychology. Founded 1936.

Psychological Record—Bloomington, Ind.; Principia Press. Subscription $4.00. 500 pages annually. Edited by J. R. Kantor and C. M. Louttit. Without fixed dates, each number a single research. General psychology. Founded 1937.

Journal of Consulting Psychology—Lancaster, Penn.; Science Printing Co. Subscription $3.00. 240 pages annually. Edited by J. P. Symonds. Bi-monthly. Founded 1937.